PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXI.

PART I.

LONDON,

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MDCCCXXI.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS.

APPENDIX.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1820, to Professor JOHN CHRISTIAN OERSTED, of Copenhagen, for his Electro-magnetic Discoveries.

PHILOSOPHICAL

TRANSACTIONS.

I. *On the black rete mucosum of the Negro, being a defence against the scorching effect of the sun's rays.* *By Sir* EVERARD HOME, *Bart. F. R. S.*

Read November 9, 1820.

To ascertain the use of the black colour of the rete mucosum in the Negro, has occupied the attention of many physiologists; and I confess that this subject formed the first investigation in which I ever engaged. Fruitless, indeed, were my attempts; and when I learnt that black surfaces absorbed heat, and raised the temperature several degrees beyond any others, I gave the matter up in despair. Two years ago my attention was again called to this enquiry, upon being told by our late excellent President, that a silver fish, in a pond at Spring Grove, during a very hot summer, immediately after some trees by which the pond was shaded were cut down, was so much exposed to the sun's rays as to have its back scorched, the surface putting on the same appearance as after a burn, and rising above the scales of the

surrounding skin. I saw the fish several times, and directions were given to send it to me when it died; but I was not so fortunate as to receive it.

This extraordinary circumstance brought to my recollection one not less so. In crossing the Tropic in April, 1781, at twelve o'clock at noon, in a voyage to the West Indies, I had fallen asleep upon deck, lying upon my back, having a thin linen pair of trowsers on, and I had not slept half an hour, when I was awakened by the bustle attending the demand of forfeits on crossing the Line, and found the inside of the upper part of both thighs scorched, the effects of which have never gone off, but till now I could not imagine how it happened, always suspecting it to be the effect of the bites of insects; but I never satisfied myself upon that subject.

The effect of the sun's rays upon the fish under water, led me to suspect the mixture of light and heat to be the cause of this scorching effect.

To ascertain the truth of this opinion, I made the following experiments.

Experiment 1.

In August, 1820, I exposed the back of my hand to the sun at twelve o'clock, with a thermometer attached to it, another thermometer being placed upon a table, with the same exposure. That on my hand stood at 90°, the other at 102°. In 45 minutes blisters rose, and coagulable lymph was exuded, which became vascular under my eye: the pain was very severe.

Experiment 2.

I exposed my face, my eyelids, and the back of my hand to water heated to 120°: in a few minutes they became pain-

ful; and when the heat was further increased, I could not bear it.

Experiment 3.

I exposed the backs of my two hands to the sun's rays, with a thermometer upon each; the one hand was uncovered, the other had a covering of black cloth, under which the ball of the thermometer was placed. After ten minutes, the degree of heat of each thermometer was marked, and the appearance on the skin examined. This was repeated at three different times. The

1st time the thermometer under the cloth	91°, the other	85°
2nd time - - - - -	94 - -	91
3rd time - - - - -	106 - -	98

In every one of these trials the skin was scorched that was uncovered; the other had not suffered in the slightest degree; there was no appearance of perspiration on either hand.

Experiment 4.

The back of a Negro's hand was exposed to the sun with a thermometer upon it, which stood at 100°; at the end of ten minutes the skin had not suffered in the least.

Experiment 5.

During the eclipse of the sun on September 7, 1820, I exposed the back of my hand to the rays concentrated by a double lens of half an inch focus, at three different periods of the eclipse. When the heat to a thermometer was 75°, that is from 47 to 57 minutes past one'clock, (including three of the figures in the annexed drawing, made by Mr. BAUER, Pl. I.)

the concentrated rays felt warm, but gave no pain, although applied for ten minutes.

When the heat to a thermometer was 79°, that is at 15 minutes past two o'clock (including the twelfth figure in the annexed drawing), the concentrated rays in four minutes gave pain; in five minutes blistered the skin, and produced dots of coagulable lymph, which became vascular under the eye.

When the heat to a thermometer was 82°, that is at half past two o'clock, (including the 13th figure of the drawing), the concentrated rays in three minutes gave pain; in four, the part was blistered, and the pain could not longer be endured.

Experiment 6.

September 8th, 1820, at eleven o'clock, the heat in the sun 90°; the concentrated rays applied to my naked arm produced a vesicle. This experiment was repeated when the heat was 84°, and in seven minutes a blister formed on the arm.

Experiment 7.

September 9th, eleven o'clock, the thermometer in the sun at 90°. The concentrated rays applied to a piece of black kerseymere cloth, made tight round my arm for 15 minutes, gave no real pain, and left no impression whatever on the skin, although the nap of the cloth had been destroyed.

This experiment was repeated with white kerseymere, the heat at 86°; in 15 minutes a blister was formed.

Repeated with Irish linen, the thermomenter 86°. In 15 minutes a blister was formed, and coagulable lymph thrown out, which had become vascular.

The same experiment was made with a white handkerchief loose upon the hand, the heat 83°. In 15 minutes an inflammatory blush was produced over a surface of several inches extent, which almost immediately disappeared on withdrawing the hand from the sun's rays.

Experiment 8.

September 12th. The sun's heat at noon 85°. The concentrated rays applied to the back of the hand of a Negro from Grenada for 15 minutes, produced no visible effect; at the first moment he felt a stab going inwards, but that went off, and afterwards he had no pain.

From these experiments, it is evident that the power of the sun's rays to scorch the skin of animals is destroyed when applied to a black surface, although the absolute heat, in consequence of the absorption of the rays, is greater.

The same wise providence which has given so extraordinary a provision to the Negro for the defence of his skin, while living within the tropics, has extended it to the bottom of the eye, which otherwise would suffer in a greater or less degree when exposed to strong light; the retina, from its transparency, allowing it to pass through without injury.

That the nigrum pigmentum is not necessary for vision, but only provided as a defence against strong light, is proved by its being darker in the Negro than the European, and being of a lighter colour in fair people than in dark, and therefore lightest in those countries farthest removed from the effects of the sun.

In the monkey it is dark, and in all animals that look upwards.

In all birds exposed to the sun's rays the nigrum

pigmentum is black. In fishes, the basking shark, which lies upon the surface of the ocean, has a nigrum pigmentum. The turbot and skate, which lie upon banks of sand in shallow water, have nigrum pigmentum.

In all ruminating animals and birds of prey, there is a lucid tapetum at the bottom of the eye.

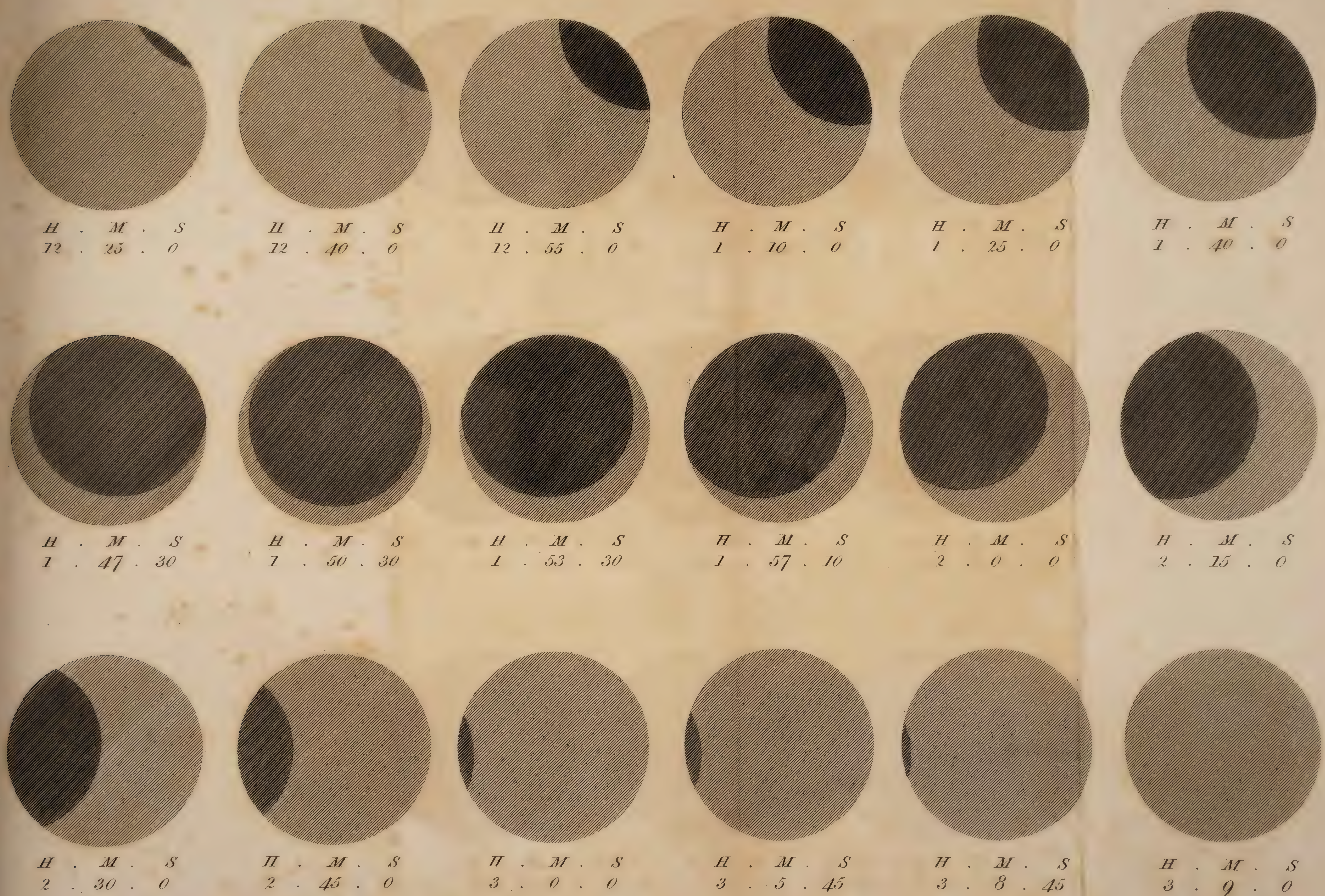
The owl, that never sees the sun, has no nigrum pigmentum.

The mackarel has the bottom of the eye lucid as quicksilver.

The *Coup de Soleil*, met with in the West Indies, the effects of which I have seen, I attribute to the scorching effect of the sun's rays upon the scalp.

The Egyptian ophthalmia I consider to be the effect of the sun's rays, and the glare of reflected light.

I have stated the fact of the scorching power of the sun's rays being destroyed when they are applied to black surfaces, but have not gone farther. Sir Humphry Davy, to whom I showed these observations, immediately explained it. He said the radiant heat in the sun's rays was absorbed by the black surface, and converted into sensible heat.



...nt Francis Bauer.

Jas Basire sc.

II. *On the magnetic phenomena produced by Electricity; in a letter from Sir* H. DAVY, *Bart. F. R. S. to* W. H. WOLLASTON, *M. D. P. R. S.*

Read November 16, 1820.

MY DEAR SIR,

THE similarity of the laws of electrical and magnetic attraction has often impressed philosophers; and many years ago, in the progress of the discoveries made with the voltaic pile, some enquirers (particularly M. RITTER,*) attempted to establish the existence of an identity or intimate relation between these two powers; but their views being generally

* M. RITTER asserted that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet; and that a metallic wire, after being exposed in the voltaic circuit, took a direction N. E. and S. E. His ideas are so obscure, that it is often difficult to understand them; but he seems to have had some vague notion that electrical combinations, when not exhibiting their electrical tension, were in a magnetic state, and that there was a kind of electro-magnetic meridian depending upon the electricity of the earth. See Annales de Chimie, T. 64, p. 80. Since this letter has been written, Dr. MARCET has been so good as to send me from Genoa, some pages of ALDINI on Galvanism, and of IZARN's Manual of Galvanism, published at Paris more than sixteen years ago. M. MOJON, senior, of Genoa, is quoted in these pages as having rendered a steel needle magnetic, by placing it in a voltaic circuit for a great length of time. This, however, seems to have been dependent merely upon its place in the magnetic meridian, or upon an accidental curvature of it; but M. ROMAGNESI, of Trente, is stated to have discovered that the pile of Volta caused a declination of the needle; the details are not given, but if the general statement be correct, the author could not have observed the same fact as M. OERSTED, but merely supposed that the needle had its magnetic poles altered after being placed in the voltaic circuit as a part of the electrical combination.

obscure, or their experiments inaccurate, they were neglected: the chemical and electrical phenomena exhibited by the wonderful combination of Volta, at that time almost entirely absorbed the attention of scientific men; and the discovery of the fact of the true connection between electricity and magnetism, seems to have been reserved for M. OERSTED, and for the present year.

This discovery, from its importance and unexpected nature, cannot fail to awaken a strong interest in the scientific world; and it opens a new field of enquiry, into which many experimenters will undoubtedly enter: and where there are so many objects of research obvious, it is scarcely possible that similar facts should not be observed by different persons. The progress of science is, however, always promoted by a speedy publication of experiments; hence, though it is probable that the phenomena which I have observed may have been discovered before, or at the same time in other parts of Europe, yet I shall not hesitate to communicate them to you, and through you to the Royal Society.

I found, in repeating the experiments of M. OERSTED with a voltaic apparatus of one hundred pair of plates of four inches, that the south pole of a common magnetic needle (suspended in the usual way) placed under the communicating wire of platinum, (the positive end of the apparatus being on the right hand) was strongly attracted by the wire, and remained in contact with it, so as entirely to alter the direction of the needle, and to overcome the magnetism of the earth. This I could only explain by supposing that the wire itself became magnetic during the passage of the electricity through it, and direct experiments, which I immediately made, proved that this was the case. I threw

some iron filings on a paper, and brought them near the communicating wire, when immediately they were attracted by the wire, and adhered to it in considerable quantities, forming a mass round it ten or twelve times the thickness of the wire: on breaking the communication, they instantly fell off, proving that the magnetic effect depended entirely on the passage of the electricity through the wire. I tried the same experiment on different parts of the wire, which was seven or eight feet in length, and about the twentieth of an inch in diameter, and I found that the iron filings were every where attracted by it; and making the communication with wires between different parts of the battery, I found that iron filings were attracted, and the magnetic needle affected in every part of the circuit.

It was easy to imagine that such magnetic effects could not be exhibited by the electrified wire without being capable of permanent communication to steel. I fastened several steel needles, in different directions, by fine silver wire to a wire of the same metal, of about the thirtieth of an inch in thickness and eleven inches long, some parallel, others transverse, above and below in different directions: and I placed them in the electrical circuit of a battery of thirty pairs of plates of nine inches by five, and tried their magnetism by means of iron filings: they were all magnetic: those which were parallel to the wire attracted filings in the same way as the wire itself, but those in transverse directions exhibited each two poles, which being examined by the test of delicate magnets, it was found that all the needles that were placed under the wire (the positive end of the battery being east) had their north poles on the south side of the wire, and their

south poles on the north side; and that those placed over, had their south poles turned to the south, and their north poles turned to the north; and this was the case whatever was the inclination of the needles to the horizon. On breaking the connection, all the steel needles that were on the wire in a transverse direction retained their magnetism, which was as powerful as ever, whilst those which were parallel to the silver wire appeared to lose it at the same time as the wire itself.

I attached small longitudinal portions of wires of platinum, silver, tin, iron, and steel, in tranverse directions, to a wire of platinum that was placed in the circuit of the same battery. The steel and the iron wire immediately acquired poles in the same manner as in the last experiment; the other wires seemed to have no effect, except in acting merely as parts of the electrical circuit; the steel retained its magnetism as powerfully after the circuit was broken as before; the iron wire immediately lost a part of its polarity, and in a very short time the whole of it.

The battery was placed in different directions as to the poles of the earth; but the effect was uniformly the same. All needles placed transversely under the communicating wires, the positive end being on the right hand, had their north poles turned towards the face of the operator, and those above the wire their south poles; and on turning the wire round to the other side of the battery, it being in a longitudinal direction, and marking the side of the wire, the same side was always found to possess the same magnetism; so that in all arrangements of needles transversely round the wire, all the needles above had north and south poles oppo-

site to those below, and those arranged vertically on one side, opposite to those arranged vertically on the other side.

I found that contact of the steel needles was not necessary, and that the effect was produced instantaneously by the mere juxta-position of the needle in a transverse direction, and that through very thick plates of glass: and a needle that had been placed in a transverse direction to the wire merely for an instant, was found as powerful a magnet as one that had been long in communication with it.

I placed some silver wire of $\frac{1}{20}$ of an inch and some of $\frac{1}{50}$, in different parts of the voltaic circuit when it was completed, and shook some steel filings on a glass plate above them: the steel filings arranged themselves in right lines always at right angles to the axis of the wire; the effect was observed, though feebly, at the distance of a quarter of an inch above the thin wire, and the arrangement in lines was nearly to the same length on each side of the wire.

I ascertained by several experiments, that the effect was proportional to the quantity of electricity passing through a given space, without any relation to the metal transmitting it: thus, the finer the wires the stronger their magnetism.

A zinc plate of a foot long and six inches wide arranged with a copper plate on each side, was connected, by a very fine wire of platinum, according to your method; and the plates were plunged an inch deep in diluted nitric acid. The wire did not sensibly attract fine steel filings. When they were plunged two inches, the effect was sensible; and it increased with the quantity of immersion. Two arrangements of this kind acted more powerfully than one; but when the two were combined so as to make the zinc and copper-plates but

parts of one combination, the effect was very much greater. This was shown still more distinctly in the following experiment. Sixty zinc plates with double copper-plates were arranged in alternate order, and the quantity of iron filings which a wire of a determinate thickness took up observed: the wire remaining the same, they were arranged so as to make a series of thirty; the magnetic effect appeared more than twice as great; that is, the wire raised more than double the quantity of iron filings.

The magnetism produced by voltaic electricity seems (the wire transmitting it remaining the same) exactly in the same ratio as the heat; and however great the heat of a wire, its magnetic powers were not impaired. This was distinctly shown in transmitting the electricity of twelve batteries of ten plates each of zinc, with double copper arranged as three, through fine platinum wire, which when so intensely ignited as to be near the point of fusion, exhibited the strongest magnetic effects, and attracted large quantities of iron filings and even small steel needles from a considerable distance.

As the discharge of a considerable quantity of electricity through a wire seemed necessary to produce magnetism, it appeared probable, that a wire electrified by the common machine would not occasion a sensible effect; and this I found was the case, on placing very small needles across a fine wire connected with a prime conductor of a powerful machine and the earth. But as a momentary exposure in a powerful electrical circuit was sufficient to give permanent polarity to steel, it appeared equally obvious, that needles placed transversely to a wire at the time that the electricity of a common Leyden battery was discharged through it,

ought to become magnetic; and this I found was actually the case, and according to precisely the same laws as in the voltaic circuit; the needle *under* the wire, the positive conductor being on the right hand, offering its north pole to the face of the operator, and the needle *above*, exhibiting the opposite polarity.

So powerful was the magnetism produced by the discharge of an electrical battery of 17 square feet highly charged, through a silver wire of $\frac{1}{20}$ of an inch, that it rendered bars of steel of two inches long and from $\frac{1}{20}$ to $\frac{1}{10}$ in thickness, so magnetic, as to enable them to attract small pieces of steel wire or needles; and the effect was communicated to a distance of five inches above or below or laterally from the wire, through water or thick plates of glass or metal electrically insulated.

The facility with which experiments were made with the common Leyden battery, enabled me to ascertain several circumstances which were easy to imagine, such as that a tube filled with sulphuric acid of $\frac{1}{4}$ of an inch in diameter, did not transmit sufficient electricity to render steel magnetic; that a needle placed transverse to the explosion through air, was less magnetized than when the electricity was passed through wire; that steel bars exhibited no polarity (at least at their extremities) when the discharge was made through them as part of the circuit, or when they were placed parallel to the discharging wire; that two bars of steel fastened together, and having the discharging wire placed through their common centre of gravity, showed little or no signs of magnetism after the discharge till they were separated, when they exhibited their north and south poles opposite to each other, according to the law of position.

These experiments distinctly showed, that magnetism was produced whenever concentrated electricity passed through space; but the precise circumstances, or law of its production, were not obvious from them. When a magnet is made to act on steel filings, these filings arrange themselves in curves round the poles, but diverge in right lines; and in their adherence to each other form right lines, appearing as spicula. In the attraction of the filings round the wire in the voltaic circuit, on the contrary, they form one coherent mass, which would probably be perfectly cylindrical were it not for the influence of gravity. In first considering the subject, it appeared to me that there must be as many double poles as there could be imagined points of contact round the wire; but when I found the N. and S. poles of a needle uniformly attracted by the same quarters of the wire, it appeared to me that there must be four principal poles corresponding to these four quarters. You, however, pointed out to me that there was nothing definite in the poles, and mentioned your idea, that the phenomena might be explained, by supposing a kind of revolution of magnetism round the axis of the wire, depending for its direction upon the position of the negative and positive sides of the electrical apparatus.

To gain some light upon this matter, and to ascertain correctly the relations of the north and south poles of steel magnetized by electricity to the positive and negative state, I placed short steel needles round a circle made on paste-board, of about two inches and half in diameter, bringing them near each other, though not in contact, and fastening them to the paste-board by thread, so that they formed the sides of a hexagon inscribed within the circle. A wire was

fixed in the centre of this circle, so that the circle was parallel to the horizon, and an electric shock was passed through the wire, its upper part being connected with the positive side of a battery, and its lower part with the negative. After the shock all the wires were found magnetic, and each had two poles; the south pole being opposite to the north pole of the wire next to it, and vice versa; and when the north pole of a needle was touched with a wire, and that wire moved round the circle to the south pole of the same needle, its motion was opposite to that of the apparent motion of the sun.

A similar experiment was tried with six needles arranged in the same manner; with only this difference, that the wire positively electrified was below. In this case the results were precisely the same, except that the poles were reversed; and any body, moved in the circle from the north to the south pole of the same needle, had its direction from east to west.

A number of needles were arranged as polygons in different circles round the same piece of paste-board, and made magnetic by electricity; and it was found that in all of them, whatever was the direction of the paste-board, whether horizontal or perpendicular, or inclined to the horizon, and whatever was the direction of the wire with respect to the magnetic meridian, the same law prevailed; for instance, when the positive wire was east, and a body was moved round the circle from the north to the south poles of the same wire; its motion (beginning with the lower part of the circle) was from north to south, or with the upper part from south to north; and when the needles were arranged round a cylinder of paste-board so as to cross the wire, and a pencil mark drawn in the direction of the poles, it formed a spiral.

It was perfectly evident from these experiments, that as many polar arrangements may be formed as chords can be drawn in circles surrounding the wire ; and so far these phenomena agree with your idea of revolving magnetism ; but I shall quit this subject, which I hope you will yourself elucidate for the information of the Society, to mention some other circumstances and facts belonging to the enquiry.

Supposing powerful electricity to be passed through two, three, four, or more wires, forming part of the same circuit parallel to each other in the same plane, or in different planes, it could hardly be doubted that each wire, and the space around it, would become magnetic in the same manner as a single wire, though in a less degree; and this I found was actually the case. When four wires of fine platinum were made to complete a powerful voltaic circuit, each wire exhibited its magnetism in the same manner, and steel filings on the sides of the wires opposite attracted each other.

As the filings on the opposite sides of the wire attracted each other in consequence of their being in opposite magnetic states, it was evident, that if the similar sides could be brought in contact, steel filings upon them would repel each other.—This was very easily tried with two voltaic batteries arranged parallel to each other, so that the positive end of one was opposite to the negative end of the other : steel filings upon two wires of platinum joining the extremities strongly repelled each other. When the batteries were arranged in the *same* order, *i. e.* positive opposite to positive, they attracted each other ; and wires of platinum (without filings) and fine steel wire (still more strongly) exhibited similar phenomena of attraction and repulsion under the same circumstances.

As bodies magnetized by electricity put a needle in motion, it was natural to infer that a magnet would put bodies magnetized by electricity in motion; and this I found was the case. Some pieces of wire of platinum, silver, and copper, were placed separately upon two knife edges of platinum connected with two ends of a powerful voltaic battery, and a magnet presented to them; they were all made to roll along the knife edges, being attracted when the north pole of the magnet was presented, the positive side of the battery being on the right hand, and repelled when it was on the left hand; and vice versa, changing the pole of the magnet. Some folds of gold leaf were placed across the same apparatus, and the north pole of a powerful magnet held opposite to them; the folds approached the magnet, but did not adhere to it. On the south pole being presented, they receded from it.

I will not indulge myself by entering far into the theoretical part of this subject; but a number of curious speculations cannot fail to present themselves to every philosophical mind, in consequence of the facts developed; such as whether the magnetism of the earth may not be owing to its electricity, and the variation of the needle to the alterations in the electrical currents of the earth in consequence of its motions, internal chemical changes, or its relations to solar heat; and whether the luminous effects of the auroras at the poles are not shown, by these new facts, to depend on electricity. This is evident, that if strong electrical currents be supposed to follow the apparent course of the sun, the magnetism of the earth ought to be such as it is found to be.

But I will quit conjectures, to point out a simple mode of making powerful magnets, namely, by fixing bars of steel

across, or circular pieces of steel fitted for making horse-shoe magnets, round the electrical conductors of buildings in elevated and exposed situations.*

The experiments detailed in these pages were made with the apparatus belonging to the Royal and London Institution; and I was assisted in many of them by Mr. PEPYS, Mr. ALLEN, and Mr. STODART, and in all of them by Mr. FARADAY.†

I am, my dear Sir,

very sincerely yours,

HUMPHRY DAVY.

Lower Grosvenor Street,
Nov. 12, 1820.

* There are many facts recorded in the Philosophical Transactions which prove the magnetizing powers of lightning; one in particular, where a stroke of lightning passing through a box of knives, rendered most of them powerful magnets. See Philosophical Transactions, No. 157, p. 520; and No. 437, p. 57.

† All the experiments detailed in this paper, except those mentioned p. 15, were made in the course of October, 1820; the last arose in consequence of a conversation with Dr. WOLLASTON, and were made in the beginning of November. I find, by the Annales de Chimie et de Physique, for September, which arrived in London November 24, that M. ARAGO has anticipated me in the discovery of the attractive and magnetizing powers of the wires in the voltaic circuit; but the phenomena presented by the action of common electricity (which I believe as yet have been observed by no other person), induce me still to submit my paper to the Council of the Royal Society. Before any notice arrived of the researches of the French philosophers, I had tried, with Messrs. ALLEN and PEPYS, an experiment, which M. ARAGO likewise thought of,—whether the arc of flame of the voltaic battery would be affected by the magnet; but from the imperfection of our apparatus, the results were not decisive. I hope soon to be able to repeat it under new circumstances.

I have made various experiments, with the hope of affecting electrified wires by the magnetism of the earth, and of producing chemical changes by magnetism; but without any successful results.

Since I have perused M. AMPERE's elaborate treatise on the electro-magnetic phenomena, I have passed the electrical shock along a spiral wire twisted round a glass

tube containing a bar of steel, and I found that the bar was rendered powerfully magnetic by the process.

Without meaning to offer any decided opinion on that Gentleman's ingenious views, I shall beg permission to mention two circumstances, which seem to me unfavourable to the idea of the identity of electricity and magnetism; 1st. the great distance to which magnetism is communicated by common electricity (I found that a steel bar was made magnetic at 14 inches distance from a wire transmitting an electric shock from about 70 feet of charged surface); and, 2d. that the effect of magnetizing at a distance by electricity takes place with the same readiness through air and water, glass, mica, or metals; *i. e.* through conductors and non-conductors.

III. *A Communication of a singular fact in Natural History. By the Right Honourable the Earl of* MORTON, *F. R. S. in a Letter addressed to the President.*

Read November 23, 1820.

MY DEAR SIR,

I YESTERDAY had an opportunity of observing a singular fact in Natural History, which you may perhaps deem not unworthy of being communicated to the Royal Society,

Some years ago, I was desirous of trying the experiment of domesticating the Quagga, and endeavoured to procure some individuals of that species. I obtained a male; but being disappointed of a female, I tried to breed from the male quagga and a young chesnut mare of seven-eighths Arabian blood, and which had never been bred from: the result was the production of a female hybrid, now five years old, and bearing, both in her form and in her colour, very decided indications of her mixed origin. I subsequently parted with the seven-eighths Arabian mare to Sir GORE OUSELEY, who has bred from her by a very fine black Arabian horse. I yesterday morning examined the produce, namely, a two-years old filly, and a year-old colt. They have the character of the Arabian breed as decidedly as can be expected, where fifteen-sixteenths of the blood are Arabian; and they are fine specimens of that breed; but both in their colour, and in the hair of their manes, they have a striking resemblance to the quagga. Their colour is bay, marked more or less ke the quagga in a darker tint. Both

are distinguished by the dark line along the ridge of the back, the dark stripes across the fore-hand, and the dark bars across the back part of the legs. The stripes across the fore-hand of the colt are confined to the withers, and to the part of the neck next to them; those on the filly cover nearly the whole of the neck and the back, as far as the flanks. The colour of her coat on the neck adjoining to the mane is pale, and approaching to dun, rendering the stripes there more conspicuous than those on the colt. The same pale tint appears in a less degree on the rump; and in this circumstance of the dun tint also she resembles the quagga.

The colt and filly were taken up from grass for my inspection, and, owing to the present state of their coats, I could not ascertain whether they bear any indications of the spots on the rump, the dark pasterns, or the narrow stripes on the forehead, with which the quagga is marked. They have no appearance of the dark line along the belly, or of the white tufts on the sides of the mane. Both their manes are black; that of the filly is short, stiff, and stands upright, and Sir Gore Ouseley's stud groom alleged that it never was otherwise. That of the colt is long, but so stiff as to arch upwards, and to hang clear of the sides of the neck; in which circumstance it resembles that of the hybrid. This is the more remarkable, as the manes of the Arabian breed hang lank, and closer to the neck than those of most others. The bars across the legs, both of the hybrid and of the colt and filly, are more strongly defined, and darker than those on the legs of the quagga, which are very slightly marked; and though the hybrid has several quagga marks, which the colt and filly have not, yet the most striking, namely, the

stripes on the fore-hand, are fewer and less apparent than those on the colt and filly. These circumstances may appear singular; but I think you will agree with me, that they are trifles compared with the extraordinary fact of so many striking features, which do not belong to the dam, being in two successive instances, communicated through her to the progeny, not only of another sire, who also has them not, but of a sire belonging probably to another species; for such we have very strong reason for supposing the quagga to be.

I am, my dear Sir,

Your faithful humble servant,

Dr. W. H. Wollaston. Morton.

P. S. I have requested Sir Gore Ouseley to send me some specimens of hair from the manes of the sire, dam, colt, and filly; and I shall write to Scotland for specimens from those of the quagga and of the hybrid.

I am not apt to build hypotheses in a hurry, and have no predilection either for or against the old doctrine of impressions produced by the imagination; but I can hardly suppose that the imagination could pass by the white tufts on the quagga's mane, and attach itself to the coarseness of its hair.

Wimpole Street, August 12th, 1820

Note by Dr. Wollaston.

By the kindness of Sir Gore Ouseley, I had an opportunity of seeing the mare, the Arabian horse, the filly, and the colt, and of witnessing how correctly they agreed with the description given of them by Lord Morton.

Having shortly afterwards described the circumstances to my friend Mr. Giles, I found that he had observed some facts of nearly equal interest, of which, at my request, he has since sent me the following account.

IV. *Particulars of a fact, nearly similar to that related by Lord* Morton, *communicated to the President, in a letter from* Daniel Giles, *Esq.*

Read November 23, 1820.

In answer to your enquiries, I will now give the best account I can of my sow and her produce.

She was one of a well known black and white breed of Mr. Western, the Member for Essex. About ten years since I put her to a boar of the wild breed, and of a deep chesnut colour, which I had just received from Hatfield House, and which was soon afterwards drowned by accident. The pigs produced (which were her first litter) partook in appearance of both boar and sow, but in some the chesnut colour of the boar strongly prevailed.

The sow was afterwards put to a boar of Mr. Western's breed (the wild boar having been long dead). The produce was a litter of pigs, some of which we observed, with much surprize, to be stained and clearly marked with the chesnut colour which had prevailed in the former litter.

This sow had afterwards another litter of pigs by a boar of Mr. Western's breed, and I think, and so does my bailiff, that some of these were also slightly marked with the chesnut colour; but though we noticed the recurrence with surprize, it is so long since, that our recollection is much less perfect than I wish it to be.

I should observe, that I have known Mr. WESTERN's breed many years, but never in any other instance observed the least appearance of the chesnut colour.

Believe me, &c.

DANIEL GILES.

Youngsbury, Nov. 10, 1820.

V. *The Croonian Lecture. Microscopical observations on the following subjects. On the Brain and Nerves; showing that the materials of which they are composed exist in the blood. On the discovery of valves in the branches of the vas breve, lying between the villous and muscular coats of the stomach. On the structure of the Spleen. By Sir* Everard Home, *Bart. V. P. R. S.*

Read December 7th, 1820.

The Croonian Lectures for the three preceding years, contain Mr. Bauer's microscopical observations on the blood. That fluid we find is made up of a greater number of ingredients than it was known to contain; indeed we find in it materials ready prepared, for the formation of most of the structures of an animal body.

In the present Lecture, the brain and nerves form the first subject of investigation. Having found upon a former occasion that the retina is perfectly transparent in the living body, and is only rendered visible by coagulation after death, this, the only expansion of medullary substance in the body with which I am acquainted, was examined by Mr. Bauer in the microscope.

He found the optic nerve to consist of many bundles of extremely delicate fibres, formed of minute globules connected together by a gelatinous substance, which readily dissolves in water. The dimensions of the globules, measured on the micrometer, explained in the preceding Lectures, are from $\frac{1}{2800}$ to $\frac{1}{4000}$ parts of an inch, mixed with very few

of $\frac{1}{2000}$ parts, the size of the red globules deprived of their colouring matter.

The retina appeared as a continuation of the bundles composing the optic nerve, and consists entirely of the same sized globules connected into fibres, and forming bundles, which go off distinctly from the end of the nerve, like rays: towards the circumference they almost disappear, and end in smooth membrane.

The whole retina is interwoven with innumerable blood vessels, both arteries and veins; the gelatinous substance that holds the globules together, dissolves in water very readily; so that if the parts are soaked in water for three or four days with a portion of the optic nerve, they become a mass of globules, and the blood vessels, when separated, form a beautifully delicate net-work, their branches anastomosing freely with one another. These appearances are represented in Pl. II. fig. 4. magnified 400 times.

By the discovery of this transparent substance, we become acquainted with the nature of the medullary structure of the nerves; and can form some idea of their action, which, till now, I confess myself to have been totally unacquainted with. The nerves, as well as the retina, are composed of this newly discovered transparent substance, which is very elastic, and soluble in water, and globules of $\frac{1}{2800}$ and $\frac{1}{4000}$ parts of an inch in diameter. Its transparency and solubility account for its having remained concealed; and were it not coagulable, in which state it becomes opaque, its existence might even now be considered as equivocal.

Before I say more of this transparent jelly, I will state Mr. Bauer's observations on the structure of the brain, of which

it makes an essential part. If the mass of the brain is kept in water for 48 hours, and a thin slice is cut from the medullary part of the cerebrum, and laid upon a glass plate previously wetted with water, and a drop of water is allowed to fall upon the slice, holding the glass a little obliquely, so that the water must run across the surface of the glass, the force with which it moves is sufficient to break down the medullary substance of the brain, so as to bring distinctly into view innumerable loose globules, many fragments of fibres of single rows of globules, and bundles of fibres, some of them of considerable length, as represented in Pl. II. fig. 1.

If the substance of the brain is laid upon a piece of dry glass, and the separation of its parts is attempted by instruments, it is impossible to effect it, as the viscous mucus adheres strongly to the glass, and the substance would be indistinctly daubed on the glass, in the manner of a pigment, a state in which the globules are not discernible.

It is impossible to distinguish the fibres composed of globules, in an opaque state of the substance; for, although in the section of any part of the brain, by means of a very strong magnifying lens, lines are discernible, these lines are produced by the light and shade on the substance, and only denote the bundles of fibres of which the brain is composed, but not the simple globular fibres.

The gelatinous mucus seems to dissolve readily, and mix with the water; and, being perfectly colourless and transparent, is entirely invisible while the substance of the brain is fresh, or whilst it is immersed in water; but if the water is left to evaporate, and the substance gets dry, the mucus collects round the loose globules and fibres in considerable

quantity, or forms irregular flakes or splotches upon the glass, perfectly transparent, and of a yellowish tinge, as represented in Pl. II. fig. 2.

If a portion is cut off from the brain in a fresh state, before it has been put in water, and laid upon a dry glass plate, and covered by a cup, so as to prevent evaporation, a perfectly colourless aqueous fluid is exuded, which evaporates on exposure to the air, and hardly leaves any mark upon the glass.

The cortical substance of the cerebrum contains also a fluid resembling the serum of the blood; it has a yellower tint than the fluids in the medullary substance, or any other part of the brain; and, when dry, it assumes the glassy appearance, and forms the same cracks that the serum does when dried on glass.

The above are all the visible materials that can be distinguished in the different parts of the human brain by means of the microscope; and, making allowance for slight modifications, are the same in different parts of the organ.

The globules are from $\frac{1}{2400}$ to $\frac{1}{4000}$ of an inch in diameter; but the general or predominant size is $\frac{1}{3200}$. They are semi-transparent, and of a white colour, arranged into fibres of single globules, and seem to be held together by the viscid mucus. The fibres form bundles connected in the same way.

The principal difference in the appearance of the different parts of the brain, consists in the proportions the quantity of mucus and fluids bear to the quantity of globular tissue in the same part, and in some respects in the size of the globules; as for instance, the cortical substance of the cerebrum and cerebellum, (which are in all respects alike) consists

chiefly of globules from $\frac{1}{3200}$, to $\frac{1}{4000}$ of an inch in diameter; and the smaller globules prevail. The single globular fibres are not so readily distinguished as in the other parts of the brain; the gelatinous mucus and fluid resembling serum, are very abundant. The finest and most delicate branches of the arteries and veins are only found in the cortical substance.

The medullary substance of the cerebrum and cerebellum differs from the above in the large globules prevailing; the mucus being more tenacious and less in quantity in proportion to the globular tissue, and the single globular fibres being more distinct, and the arterial and venal branches being larger.

The crura cerebri and cerebelli resemble in general the medullary substance, only that the mucus and fluids are more abundant; and there appears a greater proportion of mucus than globules; the blood vessels are larger than in the medullary substance.

The medulla oblongata, the corpora pyramidalia and olivaria have nearly the same structure as the medullary substance; the single globular fibres, and their bundles, are composed of the larger globules; the mucus, however, is very abundant, and is sooner dissolved in water than the mucus in any other part of the brain.

The pons verolii is principally composed of globules $\frac{1}{3200}$ of an inch; the fibres not quite so distinct as in the medulla oblongata; the mucus very abundant. The medulla spinalis has the globules of $\frac{1}{2400}$ to $\frac{1}{3200}$ of an inch predominant; the mucus and fluid less tenacious, but in greater quantity than in any part of the brain; for this reason, the single globular fibres are not so readily discovered; for if

the part is not sufficiently soaked in water, they cannot be separated; and if macerated too much, the whole is dissolved into a mass, like cream. The corpus callosum resembles the medulla spinalis, but contains a greater quantity of globules $\frac{1}{2400}$ of an inch than any other part of the brain; the quantity of mucus and fluid are at least equal to the globular tissue.

Every part of the substance of the brain is pervaded by innumerable blood vessels, which are of considerable size towards the centre, but branch out to an extreme degree of minuteness, less than the half diameter of a red globule with its colouring matter; and even when of that size the fluid they carry is red, as in Pl. II. fig. 4.

These arteries in the brain never anastomose, as in the retina; their branches are accompanied by veins of still less diameter, having valves. The valves are at very short distances, particularly near their extremities; and when the brain is fresh, these veins contain a red fluid. See Fig. 3.

The circumstances noticed by Mr. Bauer, namely, the cortical substance of the cerebrum and cerebellum being made up of the small globules; containing the gelatinous fluid soluble in water in great abundance; and having branches more minute than the other arteries of the brain; also the corresponding veins having valves similar to those found in absorbent vessel and their canals carrying a red fluid,—throw considerable light upon the functions of the brain, and show that the cortical substance is one of the most essential parts of this organ, although the pons verolii, as the commune vinculum between the different portions of this complicated structure, may be the most essential to life.

That the cortical part of the brain is the seat of memory, is an opinion I have long entertained, from finding that any continued undue pressure upon the upper anterior part of the brain entirely destroys memory, and a less degree materially diminishes it. Pressure upon the dura mater, where the skull has been trepanned, puts a temporary stop to all sense, which is restored the moment that pressure is removed; and the organ appears to receive no injury from repeated experiments of this kind having been made. In hydrocephalus, when the fluid is in large quantity, and there only remains the cortical part of the brain and pons verolii connecting it to the cerebellum, all the functions go on, and the memory can retain passages of poetry, so as to say them by heart; but a violent shake of the head produces instant insensibility. Pressure in a slight degree upon the sinciput, produced in one case complete derangement, with violent excesses of the passion of lust, both of which went off upon removing, by the crown of the trepan, the depressed bone.

The veins being so minute, and being supplied with valves, explains the circumstance of lymphatics never having been met with in this organ; these veins performing that office, carrying the absorbed matter into the superior longitudinal sinus, which appears more a reservoir than a vein; for the fluid that passes through it is not simply circulating blood; it contains the colouring matter in a decomposed state as black as ink, a change we shall find it undergoes in the spleen after death.

The superior longitudinal sinus may be considered as the common receptacle of the absorbent veins of the pia mater

from its triangular form, it always remains full. The aqueous liquid, by which the ventricles are filled, varying in quantity, answers the purpose of equalizing internal pressure.

As the transparent mucus not only is one of the most abundant materials of which the brain itself is composed, but is the medium by which the globules of the retina are kept together, and serves the same purpose in the medullary texture of the nerves, there can be no doubt that the communication of sensation and volition, more or less, depend upon it. And it would appear from the following case, that when parts are regenerated, they contain a sufficient quantity of this mucus to connect them with the nerves of the body, and enable them to partake of its sensibility.

A lady, who had a wound on the breast in a healing state, had a prominent spot of a black colour suddenly make its appearance on the surface; it was very tender to the touch; next day it disappeared, and the tenderness was gone. This must have been blood coagulated upon the termination of a nerve, and therefore the impression made by touching it was communicated along the nerve; but when it was absorbed, the bare nerve received a coating of coagulable lymph, and there was no more pain.

Mr. HUNTER's comprehensive mind grasped at the idea of the existence of something of this kind, although he had not arrived at a knowledge of the substance employed to produce the effect. He said, that so wonderful was the connection between the brain and every structure of the body, that it was to be explained in no other way than by considering, that the materia vitæ was every where; that it was in two forms collected into one mass in the brain, which he called coacervata;

and diffused through the body, which he termed diffusa, and the nerves communicated between them. This grand idea of Mr. Hunter's, Mr. Bauer, by his discovery of this transparent mucus, soluble in water, has realized.

To complete the investigation of this subject, it only remained to determine, whether this transparent substance, soluble in water, is actually an ingredient in the blood, or is formed after the first changes of that fluid into the solids of the body have taken place.

To ascertain this point, two ounces of blood were drawn from the arm of a healthy man, and allowed to stand at rest till all the serum separated from the coagulum, which required 36 hours; the serum was then carefully poured off, and the phial filled up with distilled water, and the changes that occurred were attended to. In 24 hours, the upper parts of the coagulum, particularly at the edges, became tumid, apparently from having imbibed some of the water. This part was of a light red colour, and semi-transparent. A small portion of it was cut off and put into a saucer with distilled water, covered over by a watch glass. In 24 hours, carbonic acid gas was seen in bubbles round the edge of the watch-glass, the colouring matter had mixed with the water, and the whole of the substance was nearly dissolved.

From this experiment, confirmed by many others, this mucus is not only readily discovered in the blood, but proves to be the medium by which the colouring matter is attached to the surface of the red globules; and therefore when these red globules are put into water, they lose their colour from the medium dissolving by which it was attached to them.

From this investigation of the blood, it appears that the principal materials of which the body is composed, are met with in the blood. The fat is by many considered a secretion; for this opinion there is however no foundation. That the fat is formed in the colon, and is thence taken up into the blood vessels, and distributed to the different parts of the body, is sufficiently proved by the mode in which adipocere is made, and by the observations on the colon in different animals, that have been long since laid before the Society. No direct experiment has been made, that I am acquainted with, for detecting the presence of fat in the blood; possibly the reason is, that its failure would prove nothing, since the blood contains an alkali with which the oil will become united. That I might not be said to have neglected this part of the enquiry, I instituted the following experiment.

Twelve ounces of blood were drawn from the arm into a glass vessel of a globular form capable of containing a pint, with a tube rising out of the globe six inches long, and half an inch in diameter; at the end of 24 hours the serum was poured off, and the vessel and tube filled up to the orifice with distilled water; after this vessel had remained 24 hours at rest, no appearance of oil took place upon the surface. The coagulum was then broken down by a long wire, which produced an immediate evolvement of carbonic acid gas, in such quantity that the water fell in the tube an inch in length; but there was no appearance of oil seen on the surface. The blood was examined for several days successively; it became very offensive, but showed no appearance of oil. In the blood of the salmon and skate, oil is met with in such quantity as to render blotting paper greasy.

In the skate, the blood globules are of a very large size, and have an oval form; the colouring matter in them is of a light yellow; they very readily change their appearance when decomposition begins to take place: at this time the oval becomes flattened, and the central part appears more dense than the margin, by which it is surrounded in the form of a ring, and when this ring dissolves, the globule it contained is seen of a spherical form, and the surrounding fluid has oil floating in it, distinguishable by the naked eye, as well as by other tests.

The globules are represented in Pl. III. fig. 5. magnified 4000 diameters.

The salts in the skate's blood must be very abundant, since they are found in it crystallized, as represented in Pl. III. fig. 6: magnified 200 times.

On the branch of the vas breve carrying the fluids from the stomach through the splenic vein to the vena portarum.

The discovery of valvular vessels in the brain, acting as absorbents in that organ, immediately led me to suspect that there must be a similar provision for carrying off the fluids taken into the stomach, whenever the quantity or quality interfered with the process of digestion. To do this by the route of the thoracic duct, was not only too circuitous to correspond with the general simplicity of the operations of nature, but was mixing these heterogeneous liquids in too crude a state, with the general circulation of the blood. That there was some unusual mode of conveying fluids from the stomach to the

urinary bladder, I have upon a former occasion established, since they arrived there when both the pylorus and thoracic duct were tied up, and the spleen was removed out of the body; but till the fact of valvular vessels supplying the office of absorbents was ascertained, any opinion respecting the route of fluids from the stomach, must continue to be entirely hypothetical.

Upon the present occasion, through Mr. Bauer's means, I am not only enabled to demonstrate vessels so constructed in the coats of the stomach, but to give abundant collateral evidence of their acting as absorbents, even more than can be produced respecting those of the brain.

It immediately suggested itself to me, that this was the probable use of the branches of the vas breve, the presence of which upon the coats of the stomach, so well supplied with veins from other trunks, is not easily accounted for.

In the first instance, with the assistance of Mr. Clift, I injected the splenic artery, and requested Mr. Bauer to ascertain, whether any minute branches, spread upon the great curvature of the stomach, in a contrary direction to those injected arteries, had valves. Such vessels were found, and quite empty. They had valves very distinctly marked: he showed them to me, so as perfectly to satisfy me of the fact. Having got thus far, I requested the assistance of Mr. Chevalier, House Surgeon to St. George's Hospital, who has given, at different times, considerable time and attention to preparing the stomach and spleen for Mr. Bauer's observations; which he has been better enabled to do, from being more in the habit of injecting the blood vessels, than students in surgery

generally are; Mr. CLIFT'S important occupations at the College depriving me of his valuable assistance. I requested Mr. CHEVALIER to inject, as minutely as possible, the branches of the splenic artery and vein going to the stomach. In one instance, he succeeded so well that the arteries were filled to the most minute branches, and some of the injection had passed into the stomach, without any apparent rupture of the vessel. No part of this coloured injection had got into the veins, which in other parts of the circulation generally happens. Between the villi and the muscular coats of the stomach there is a very fine elastic cellular membrane: it admits of being drawn out to more than three times its natural thickness; and it was by doing so, Mr. BAUER caused these smaller arteries to be exposed, and, along with them, small valvular vessels quite empty; the valves were very numerous, and nearly at equal distances. In tracing these towards the cavity, they became indistinct just as they entered the villi. These appearances are shown in Pl. III. fig. 2, 3, 4. This representation of the valves in these vessels, as well as that of the valves of the vessels in the brain, may be considered as demonstrations of the fact; and still more valuable than preparations, since the appearance can be better preserved.

To show the course of the absorbed fluids, as well as to give a clear idea of every thing connected with so *important* a discovery, a drawing of the spleen, the vas breve, and cardiac portion of the stomach, is annexed [Pl. IV;] and as the trunk of the splenic vein forms one of the trunks of the vena portæ, the liquids are directly carried to the liver,

forming a part of the materials employed in producing the bile; the remainder only returning by the vena cava to the heart.

This additional quantity of liquids passing along the splenic vein, accounts for its being five times the size of the artery, as well as for the blood in that vein having a greater proportion of serum than the blood in any other, which has been long asserted, and which I found by actual experiment to be the case; but being unable to account for it, as I can now, I was willing to admit that the mode of measuring might be erroneous.

On the structure and uses of the spleen.

In the different investigations that have been made of this organ, the following facts have been ascertained; but *still* neither the more minute texture, nor the ultimate use has been, till now, discovered.

It was known that a man could live without his spleen; but there is no satisfactory account upon record of the inconvenience he suffered from its loss.

It has been ascertained that the spleen, under different circumstances, is larger or smaller in size. In an ass after fasting two days, it was half the size that it was met with in another, killed two hours after drinking freely. In the diminished state there are no corpuscules; in the enlarged state they are very numerous.

The spleen was believed to consist of a net-work of ligamentous structure, with numerous arterial and venal branches, having cells containing small corpuscules or glands; but this

appearance vanishes when the parts are more minutely examined. Its structure was made out in the following manner.

Wednesday, August 23, 1820, at 12 o'clock, a healthy large spleen, taken from a man twenty-eight years of age, was cut into eight transverse slices, nearly of the same thickness; four of these were put into one flat dish, and four into another; both of these were filled with distilled water; no colouring matter was given out, although the surfaces had all a red colour; the cells were unusually distinct, and had a degree of uniformity in their appearance. On examining the cells under water with a common lens, they appeared full; on turning them over in the water, something colourless fell out, carrying no colouring matter along with it. The same thing happened on turning up the opposite side. This appearance is represented in Pl. VI. fig. 1. and 2.

Aug. 24th, 12 o'clock. Some colouring matter was discharged round each slice, forming a circle round it, but not in contact with the edge. It had the appearance of red serum, but not that of the globules parting with their colour; the surface lost its red colour, becoming darker. The cells were examined by a lens, when those on the upper surface appeared hollow and empty, but those on the under surface appeared full of a mucus soaked in water, and a film was spread over the surface of several of them. The water was changed every day.

25th, at 12 o'clock, both the upper and under surfaces were obscured by flocculent mucus over the cells, which were equally distinct; the red colour was discharged in greater quantity.

26th. Both surfaces had the mucus filling the cells, of a

paler red colour than the surrounding substance, looking exactly like plum-pudding stone. The cells were tumid, rising above the surface.

27th. No material change.

28th. The surface of the cells had become flat, the discharge of colouring matter considerable.

29th. The cells still flatter, and a light coloured point in the centre of each.

30th. No change, the colouring matter still discharging.

31st. The surface so slimy that the slices were very slippery; no other change, except a number of round deep black spots: in some places they appeared as if filling the orifices of divided arteries; in others, as if the surface of one or two cells was blackened

September 1st. The black spots more numerous; on the surface of the water much colouring matter, but no mucus separated.

2nd. A greater extent of black surface; more mucus.

4th. The colouring matter nearly gone; more black along the surface; mucus on both surfaces; their cells more distinct.

5th. Black colour more extended; little mucus or colouring matter.

6th. The slice become very putrid; cells as distinct as at first; more extension of black; no colouring matter; little mucus.

8th. Upper surface all black; no appearance of cells, although seen on the under surface. The black colour was produced by the colouring matter becoming putrid.

12th. The whole substance one mass of branches of vessels; every thing else dissolved.

August 29th. in the same year. The spleen of a woman (who had taken little food for some time,) hardly more than one third the size of the other, contained no cells, and consequently no corpuscules; was treated in the same manner, and the changes it underwent were the same.

These different appearances are represented in Plates VI. VII. VIII. each having a separate explanation.

The spleen, from this account, consists of blood vessels, between which there is no cellular membrane, and the interstices are filled with serum, and the colouring matter of the blood from the lateral orifices in the veins, when these vessels are in a distended state; which serum is afterwards removed by the numberless absorbents belonging to the organ, and carried into the thoracic duct by a very large absorbent trunk.

That all the apparent fibres are vascular, is proved by the representation in Pl. III. fig. 1, in which they are minutely injected; and the injection is carried into the cells, and moulded into their form. The lymph globules carry along with them into the interstices carbonic acid gas, and the mucus soluble in water, in great abundance; but no blood globules, since none are found in the cells. As soon as the lymph is at rest, the carbonic acid gas being let loose, forms the cells that surround the lymph globules, the sides of which are held together by the mucus, putting on the appearance of corpuscules without colour, and are thus mistaken for glands; the gas is absorbed by the blood in the arteries and veins.

The spleen, from this mechanism, appears to be a reservoir for the superabundant serum, lymph globules, soluble

mucus, and colouring matter, carried into the circulation immediately after the process of digestion is completed.

EXPLANATION OF THE PLATES.

Plate II.

In this Plate several small parts of glass micrometers are represented, in which the inch is divided into 400 parts in diameter, which divides the superficies into 160,000 parts; so that every object is magnified 400 times in diameter, and 160,000 in superficies.

Fig. 1. In the first square at A, are represented the globules of the cerebrum that are predominant. These, it is evident, are $\frac{1}{3200}$ part of a lineal inch. The rest of the micrometers of fig. 1. contain many loose globules of various sizes, and fragments of bundles and simple globular fibres of the medullary substance of the cerebrum in a fresh state, immersed in water.

Fig. 2. represents the same objects in a dried state, when the accumulated mucus, and some newly produced globules, become visible.

Fig. 3. represents a very small portion of the medullary substance of the cerebrum diluted with water; also displaying fragments of single globular fibres, many loose globules, and a portion of the venal branches with valves.

Fig. 4. represents a small portion of the retina of the human eye diluted with water, consisting of loose globules and globular fibres of the same size as those of the brain in its various

parts ; also a branch of an artery whose anastomoses compose a beautiful net-work almost over the whole membrane of the retina.

This representation of an arterial branch, may serve as an illustration of those that pervade every part of the substance of the human brain ; but such branches do not any where anastomose except in the retina.

Plate III.

This Plate consists of six figures. The first representing a portion of the spleen. The second, third, and fourth, the vessels with valves passing from the internal membranes of the stomach. The fifth and sixth, the blood globules and salts in the blood of the skate.

Fig. 1. A surface of $\frac{2}{16}$ parts of a square inch of a slice of the spleen of a child five years old, minutely injected for the arteries ; magnified 8 diameters. It shows the termination of the arteries in the empty cells, which are also filled with the injection, in the same manner as the corpuscules themselves are originally formed.

Fig. 2. A small slice of the coats of the cardiac portion of the human stomach, magnified 8 diameters, from a man 48 years old. The blood vessels were minutely injected soon after death, the arteries with red, the veins with yellow ; the cellular membrane, or rather the filamentous substance between the villous and muscular coat, is in this figure stretched out to more than three times its natural thickness, and it is in this space that the empty valvular vessels are distinctly shown.

Fig. 3. and 4. Portions of the small valvular vessels, showing that they vary in appearance ; magnified 400 diameters.

Fig. 5. Some globules of the blood of the skate floating in the coloured serum. The blood was taken from the heart of a fish quite alive at the time the heart was opened. In these globules the enveloping substance is quite smooth, and the globules are perfectly in the shape of eggs; and the contained spherical globules are not visible.

B. A group of seven globules floating in the serum, diluted in water, upon a piece of glass; the globules are attracted and adhere closely to each other, and become flat; the inner spherical globule is distinctly seen, the enveloping matter forming an elevated rim round it.

C. represents the same group, after having been 20 minutes in the diluted serum; the enveloping substance is dissolved; and after all the moisture is evaporated, the spherical globules appear still closer drawn together, and quite clear and distinct, and the enveloping substance almost entirely gone, leaving only some greasy marks on the glass; magnified 400 diameters.

Fig. 6. A group of crystals of salts formed on the surface of the decomposed blood of the skate, when the blood begins to putrify; magnified 200 diameters.

Plate IV.

The human stomach and spleen in their relative situation, of the natural size, from a young man of 15 years of age.

A. shows the vas breve where it joins the splenic vein. When the branches are traced to the stomach, some dip in between its coats, the others run on the surface, anastomosing with the branches of the other trunks belonging to that viscus.

Plate V.

The human spleen of the natural size, from a man 48 years of age. The arteries and veins are injected with the same colour, showing that they run in pairs, enclosed in a common theca.

Plate VI.

Represents two sections of the spleen, one prior to maceration, the second after being in distilled water three days.

Fig. 1. A transverse section of the same spleen, as represented in Pl. V. of the natural size. Upon immersing it in distilled water, the cells emptied themselves of lymph globules; and upon turning it over, the same thing happened to the opposite side.

Fig. 2. A surface of $\frac{2}{16}$ parts of a square inch of Fig. 1. showing distinctly the cells; and also a small portion of a vein laid open, exposing the perforations met with near the sinus in the concave part of the spleen; magnified 8 diameters.

Fig. 3. A transverse slice of the spleen of a boy 15 years of age. This portion had been kept in distilled water, in an open dish, for three days, when the mucus, which with the globules forms the lining of the cells of the spleen, was so much swoln, that not only all the cells were filled with it, but the mucus was raised above the surface, giving it an uneven appearance. The parts are of the natural size.

Fig. 4. A surface of $\frac{2}{16}$ parts of a square inch of the above slice, magnified 8 diameters.

Plate VII.

Two sections of the spleen in different stages of maceration.

Fig. 1. A transverse section of the same spleen, as represented in Pl. VI. fig. 1. After remaining 12 days in distilled water in an open dish the mucus is nearly dissolved, and the black spots

(which are produced by the colouring matter becoming putrid) are rapidly extending; the cells are nearly empty. Natural size.

Fig. 2. A surface $\frac{2}{16}$ parts of a square inch of the above slice, magnified 8 diameters.

Fig. 3. A transverse slice of the same spleen soaked in distilled water 20 days, when the mucus was almost wholly dissolved, and all the colouring matter discharged; but this slice having been kept in a deep glass quite full of distilled water, and closed with a glass stopple, no atmospheric air could penetrate; the mucus and colouring matter were washed out before putrefaction could take place; the remaining globular substance then was so loose, that by the least motion of the section of the spleen the margins of the cells crumbled and fell to pieces. Of the natural size.

Fig 4. A surface of $\frac{3}{16}$ parts of a square inch of the above slice, magnified 8 diameters; showing more distinctly the crumbling of the sides of the cells.

PLATE VIII

Represents a section of the spleen completely macerated.

Fig. 1. A transverse slice of the spleen of a boy ten years of age; the spleen being kept in water 48 days, the mucus and the colouring matter were entirely dissolved and discharged; and fresh water being pumped every second day upon it, the globular substance was gradually completely washed out, and the arteries, to their most minute branches, became perfectly clear and distinct, no other parts being left.

Fig. 2. A surface of $\frac{4}{16}$ parts of a square inch of the above figure, magnified 8 diameters.

Fig. 1.

A

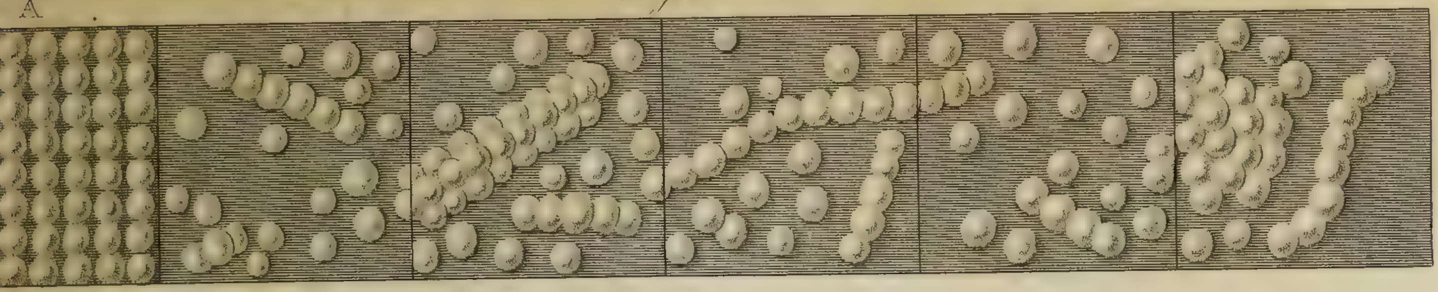


Fig. 3.

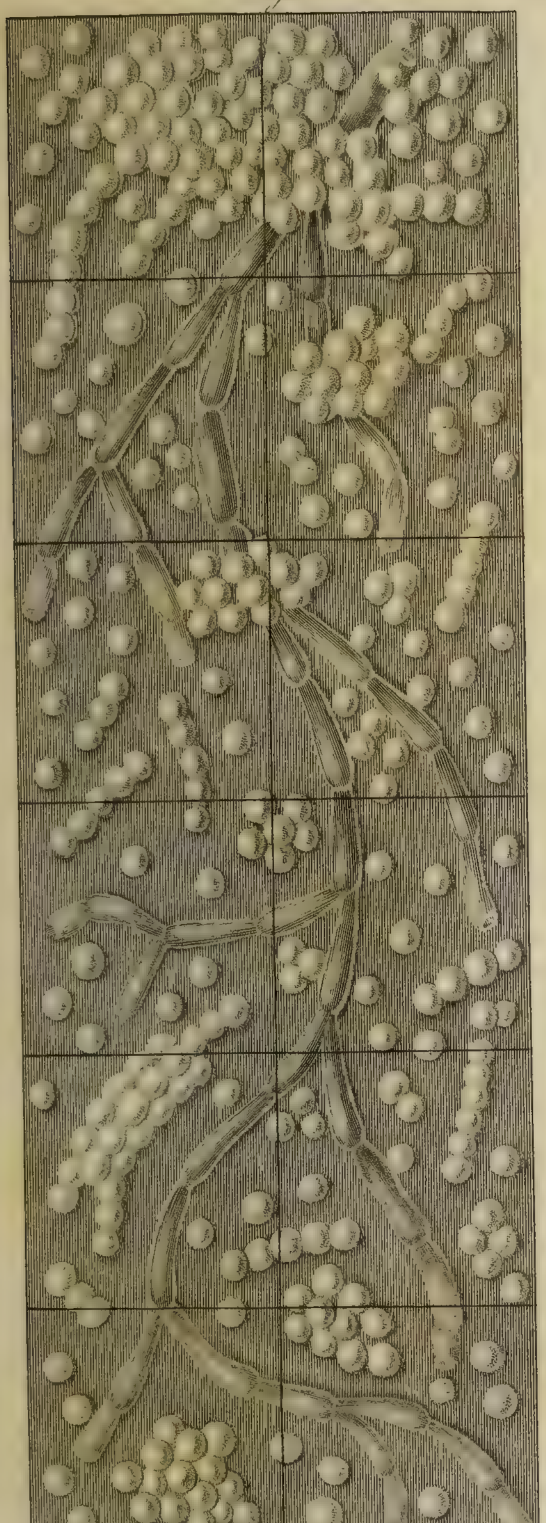


Fig. 4.

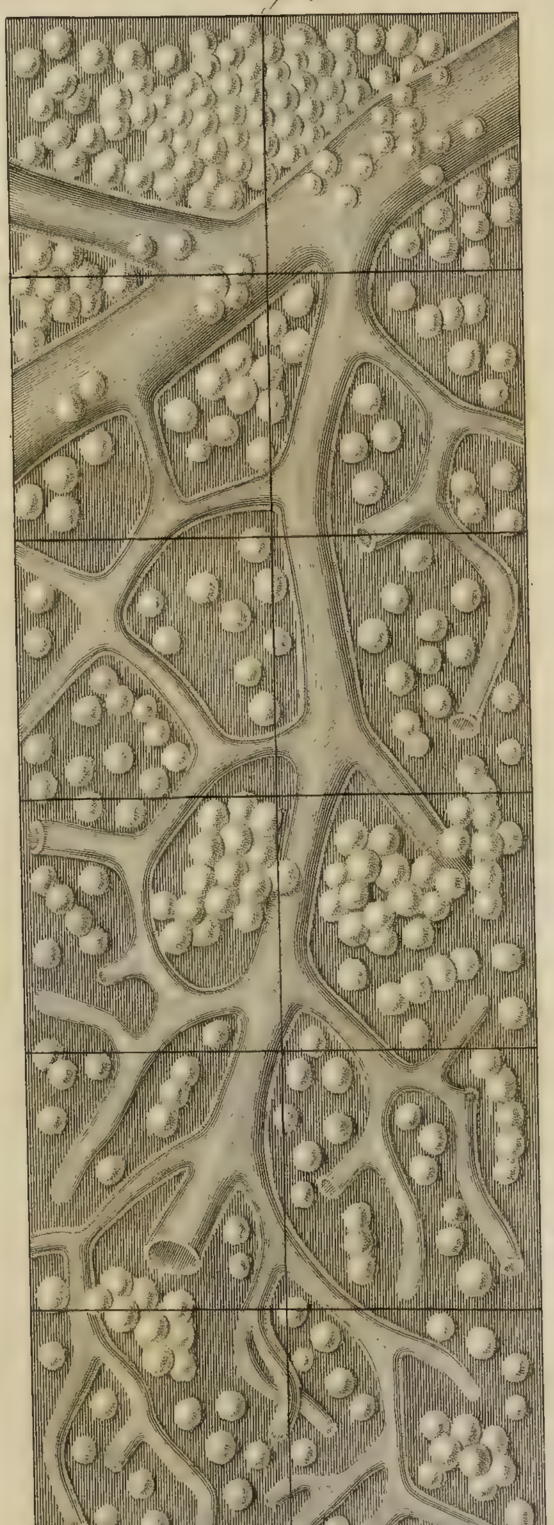


Fig. 2.

...er del.

J.s Basire sc.

Bauer del. Js. Basire sc.

Bauer del. F. Basire sc.

Fig. 1.

Fig. 2.

Fig. 3

Fig. 4.

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Fig. 1.

Fig. 2.

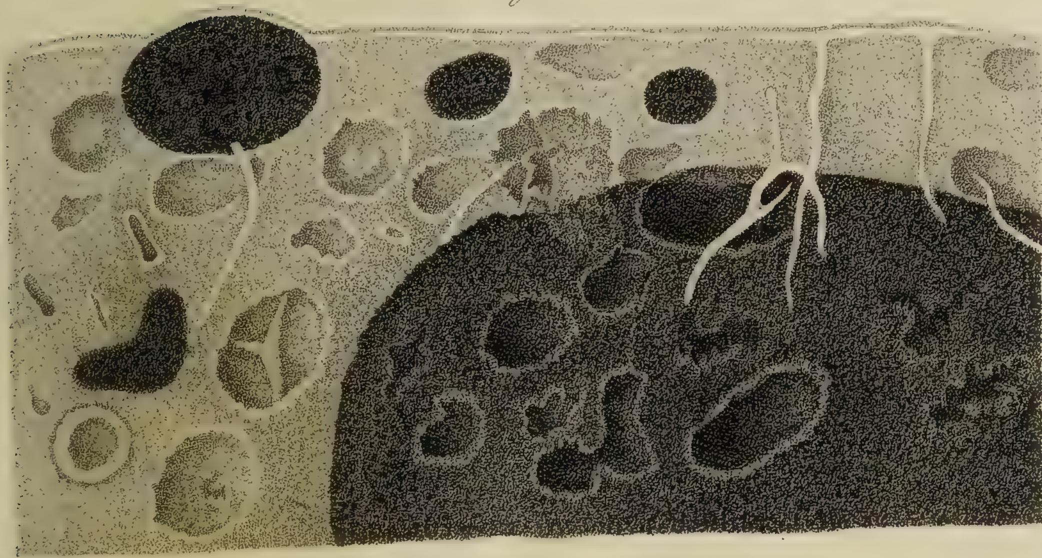


Fig. 3

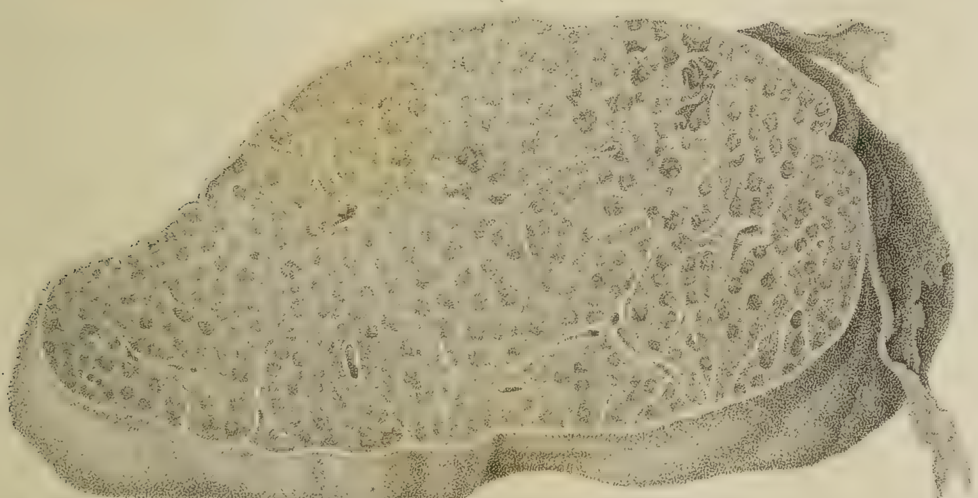
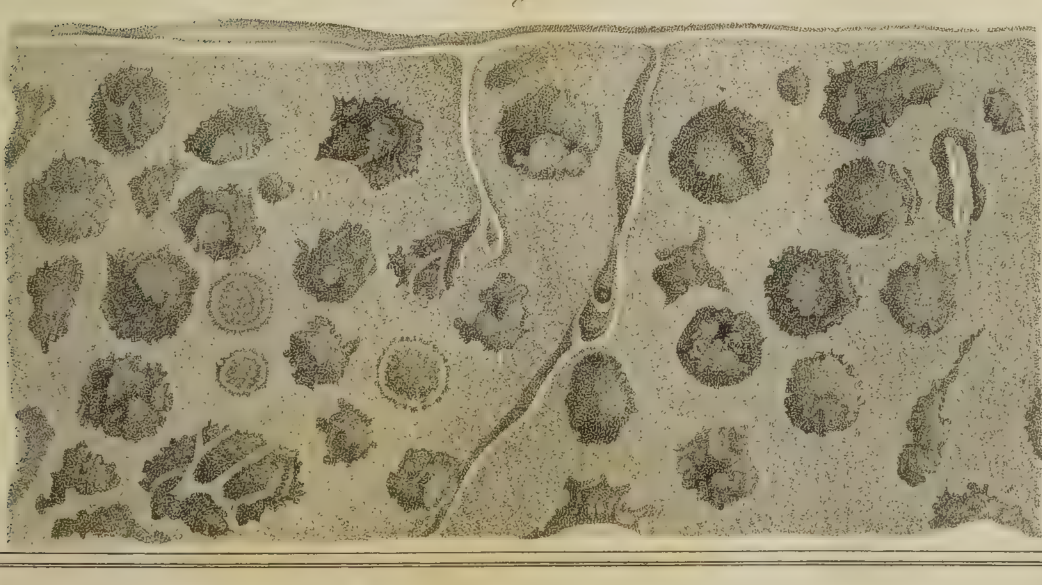


Fig. 4.



F. Bauer del.

J. Basire sc.

Fig. 1.

Fig. 2.

Bauer del. Js. Basire sc.

VI. *On two new compounds of Chlorine and Carbon, and on a new compound of Iodine, Carbon, and Hydrogen. By Mr.* FARADAY, *Chemical Assistant in the Royal Institution. Communicated by* W. T. BRANDE, *Esq. Sec. R. S. and Prof. Chem. R. I.*

Read December 21, 1820.

ONE of the first circumstances that induced Sir H. DAVY to doubt the compound nature of what was formerly called oxymuriatic acid gas, was the want of action of heated charcoal upon it; and considerable use of the same agent, and of the phenomena exhibited by it in different circumstances with chlorine, was afterwards made in establishing the simple nature of that body.

The true nature of chlorine being ascertained, it became of importance to form all the possible compounds of it with other elementary substances, and to examine them in the new view had of their nature. This investigation has been pursued with such success at different times, that very few elements remain uncombined with it; but with respect to carbon, the very circumstance which first tended to correct the erroneous opinions which, after SCHEELE's time, and before the year 1810, had gone abroad respecting its nature, proved an obstacle to the formation of its compounds; and up to the present time, the chlorides of carbon have escaped the researches of chemists.

That the difficulty met with in forming a compound of chlorine and carbon, was probably not owing to any want or

weakness of affinity between the two bodies, was pointed out by Sir H. Davy; who, reasoning on the triple compound of chlorine, carbon, and hydrogen, concluded that the attraction of the two bodies for each other was by no means feeble; and the discovery of phosgene gas by Dr. Davy, in which chlorine and carbon are combined with oxygen, was another circumstance strongly in favour of this opinion.

I was induced last summer to take up this subject, and have been so fortunate as to discover two chlorides of carbon, and a compound of iodine, carbon, and hydrogen, analogous in its nature to the triple compound of chlorine, carbon, and hydrogen, sometimes called chloric ether. I shall endeavour in the following pages to describe these substances, and give the experimental proofs of their nature.

If chlorine and olefiant gas be mixed together, it is well known that condensation takes place, and a colourless limpid volatile fluid is produced, containing chlorine, carbon, and hydrogen. If the volumes of the two gases are equal, the condensation is perfect. If the olefiant gas is in excess, that excess is left unchanged. But if the chlorine is in excess, the fluid becomes of a yellow tint, and acid fumes are produced. This circumstance alone proves, that chlorine can take hydrogen from the fluid; and on examination, I found it was without the liberation of any carbon or chlorine.

That the action thus began, might be carried to its utmost extent, some of the pure fluid (chloric ether) was put into a retort with chlorine, and exposed to sunshine. At the first instant of contact between the chlorine and the fluid, the latter became yellow; but when in the sun's rays, a few moments sufficed to destroy the colour both of the fluid and the chlorine,

heat being at the same time evolved. On opening the retort, there was no absorption, but it was found full of muriatic acid gas. This was expelled, and more chlorine introduced, and the whole again exposed to sun light: the colour again disappeared, and a few moist crystals were formed round the edge of the fluid. Chlorine being a third time introduced, and treated as before, it still removed more hydrogen; and now a sublimate of crystals lined the retort. Proceeding in this way until the chlorine exerted no farther action, the fluid entirely disappeared, and the results were, the dry crystalline substance, and muriatic acid gas.

A portion of olefiant gas was then mixed in a retort with eight or nine times its bulk of chlorine, and exposed to sun light. At first the fluid formed; but this instantly disappeared; the retort became lined with crystals, and the colour of the chlorine very much diminished.

On examining these crystals, I found they were the compound I was in search of; but before I give the proofs of their nature, I will describe the process by which this chloride of carbon can be obtained pure.

Perchloride of carbon.

A glass vessel was made in the form of an alembic head, but without the beak; the neck was considerably contracted, and had a brass cap with a stop-cock cemented on; at the top was a small aperture, into which a ground stopper fitted air tight. The capacity of the vessel was about 200 cubic inches. Being exhausted by the air-pump, it was nearly filled with chlorine; and being then placed over olefiant gas, and as much as could enter having passed in, the stop-cocks

were shut, and the whole left for a short time. When the fluid compound of chlorine and olefiant gas had formed and condensed on the sides of the vessel, it was again placed over olefiant gas, and, in consequence of the condensation of a large portion of the gases, a considerable quantity more entered. This was left, as before, to combine with part of the remaining chlorine, to condense, and to form a partial vacuum; which was again filled with olefiant gas, and the process repeated until all the chlorine had united to form the fluid, and the vessel remained full of olefiant gas. Chlorine was then admitted in repeated portions as before; consequently, more of the fluid formed; and ultimately a large portion was obtained in the bottom of the vessel, and an atmosphere of chlorine above it. It was now exposed to sun light. The chlorine immediately disappeared, and the vessel became filled with muriatic acid gas. Having ascertained that water did not interfere with the action of the substances, a small portion was admitted into the vessel which absorbed the muriatic acid gas, and then another atmosphere of chlorine was introduced. Again exposed to the light, this was partly combined with the carbon, and partly converted into muriatic acid gas; which, being as before absorbed by the water, left space for more chlorine. Repeating this action, the fluid gradually became thick and opaque from the formation of crystals in it, which at last adhered to the sides of the glass as it was turned round; and ultimately the vessel only contained chlorine with the accumulated gaseous impurities of the successive portions; a strong solution of muriatic acid, coloured blue from the solution of a little brass, and the solid substance.

I have frequently carried the process thus far in retorts;

and it is evident, that any conveniently formed glass vessel will answer the purpose. The admission of water during the process prevents the necessity of repeated exhaustion by the air-pump, which cannot be done without injury to the latter; but to have the full advantage of this part of the process, the gases should be as pure as possible, that no atmosphere foreign to the experiment may collect in the vessel.

In order to cleanse the substance, the remaining chlorine and muriatic acid were blown out of the vessel by a pair of bellows, introduced at the stoppered aperture, and the vessel afterwards filled with water, to wash away the muriatic acid and other soluble matters. Considerable care is then requisite in the farther purification of the chloride. It retains water, muriatic acid, and a substance, which I find to be a triple compound of chlorine, carbon, and hydrogen, formed from the cement of the cap; and as all these contain hydrogen, a small quantity of any one remaining with the chloride would, in analysis, give erroneous results. Various methods of purification may be devised, founded on the properties of the substance, but I have found the following the most convenient. The substance is to be washed from off the glass, and poured with the water into a jar; a little alcohol will remove the last portions which adhere to the glass; and this when poured into the water will precipitate the chloride, and the whole will fall to the bottom of the vessel. Then having decanted the water, the chloride is to be collected on a filter, and dried as much as may be by pressure between folds of bibulous paper. It should next be introduced into a glass tube, and sublimed by a spirit lamp: the pure substance with water will rise at first, but the last portions will be partially decom-

posed, muriatic acid will be liberated, and charcoal left. The sublimed portion is then to be dissolved in alcohol, and poured into a weak solution of potash, by which the substance is thrown down, and the muriatic acid neutralized and separated: then wash away the potash and muriate by repeated affusions of water, until the substance remains pure; collect it on a filter, and dry it, first between folds of paper, and afterwards by sulphuric acid in the exhausted receiver of the air-pump.

It will now appear as a white pulverulent substance; and if perfectly pure, will not, when a little of it is sublimed in a tube, leave the slightest trace of carbon, or liberate any muriatic acid. A small portion of it dissolved in ether, should give no precipitate with nitrate of silver. If it be not quite pure, it must be re-sublimed, washed, and dried until it is pure.

This substance does not require the direct rays of the sun for its formation. Several tubes were filled with a mixture of one part of olefiant gas with five or six parts of chlorine, and placed over water in the light of a dull day; in two or three hours there was very considerable absorption, and crystals of the substance were deposited on the inside of the tubes. I have also often observed the formation of the crystals in retorts in common day light.

A retort being exhausted, had 12 cubic inches of olefiant gas introduced, and 24.75 cubic inches of chlorine: as soon as the condensation occasioned by the formation of the fluid had taken place, 21.5 cubic inches more of chlorine were passed in, and the retort set aside in a dark place for two days. At the end of that time muriatic acid gas and the solid

chloride had formed, but the greater part of the fluid remained unchanged. Hence, it will form even in the dark, by length of time.

I tried to produce the chloride by exposure of the two gases in tubes over water to strong lamp light for two or three hours, but could not succeed.

The perchloride of carbon, when pure, is immediately after fusion, or sublimation, a transparent colourless substance. It has scarcely any taste. Its odour is aromatic, and approaching to that of camphor. Its specific gravity is as nearly as possible 2. Its refractive power is high, being above that of flint glass (1.5767). It is very friable, easily breaking down under pressure; and when scratched, has much of the feel and appearance of white sugar. It does not conduct electricity.

The crystals obtained by sublimation and from solutions of the substance in alcohol and ether, are dendritical, prismatic, or in plates; the varieties of form, which are very interesting, are easily ascertained, and result from a primitive octoëdron.

It volatilizes slowly at common temperatures, and passes, in the manner of camphor, towards the light. If warmed, it rises more rapidly, and then forms fine crystals: when the temperature is farther raised, it fuses at 320° F. and boils at 360°, under atmospheric pressure. When condensed again from these rapid sublimations, it concretes in the upper part of the tube or vessel containing it, in so transparent and colourless a state, that it is difficult, except from its high refractive power, to perceive where it is lodged. As the crust it forms becomes thicker, it splits, and cracks like sublimed camphor; and in a few minutes after it is cold, is white, and

nearly opaque. If the heat be raised still higher, as when the substance is passed through a red hot tube, it is decomposed, chlorine is evolved, and another chloride of carbon, which condenses into a fluid, is obtained. This shall be described presently.

It is not readily combustible; when held in the flame of a spirit lamp, it burns with a red flame, emitting much smoke and acid fumes; but when removed from the lamp, combustion ceases. In the combustion that does take place in the lamp, the hydrogen of the alcohol, by combining with the chlorine of the compound, performs the most important part; nevertheless, when the substance is heated red in an atmosphere of pure oxygen, it sometimes burns with a brilliant light.

It is not soluble in water at common temperatures; or only in very small quantity. When a drop or two of the alcoholic solution is poured into a large quantity of water, it renders it turbid from the deposition of the substance. It does not appear that hot water dissolves more of it than cold water.

It dissolves in alcohol with facility, and in much greater quantity with heat than without. A saturated hot solution crystallizes as it cools, and the cold solution also gives crystals by spontaneous evaporation. When poured into water, the chloride is precipitated, and falls to the bottom in flakes. If burnt, the flame of the alcohol is brightened by the presence of the substance, and fumes of muriatic acid are liberated. Solution of nitrate of silver does not produce any turbidness in it, unless it be in such quantity that the water throws down the substance; but no chloride of silver is formed.

It is much more soluble in ether than in alcohol, and more

so in hot than in cold ether. The hot solution deposits crystals as it cools; and the crystallization of a cold solution, when evaporated on a glass plate, is very beautiful. This solution is not precipitated by water, unless the ether has previously been dried, and then water occasions a turbidness. Nitrate of silver does not precipitate it. When burned, muriatic acid fumes are liberated, but the greater part of the chloride remains in the capsule.

It is soluble in the volatile oils, and on evaporation is again obtained in crystals. It is also readily soluble in fixed oils. The solutions when heated liberate muriatic acid gas, and the oil becomes of a dark colour, as if charred.

Solutions of the acids and alkalies do not act with any energy on the substance. When boiled with solutions of pure potash and soda, it rises and condenses in the upper part of the vessel; and though it be brought down to the alkali many times, and re-boiled, still the alkali, when examined, is not found to contain any chlorine, nor is any change produced. Ammonia in solution is also without action upon it. These solutions do not appear to dissolve more of it than pure water.

Muriatic acid in solution does not act at all upon it. Strong nitric acid boiled upon it dissolves a portion, but does not decompose it: as it cools, part of the chloride is deposited unaltered, and the concentrated acid, when diluted, lets more fall down. The diluted portion being filtered, and tested with nitrate of silver, gives no precipitate. It does not appear to be either soluble in, or acted upon, by concentrated sulphuric acid. It sinks slowly in the acid, and, when heated,

is converted into vapour, which, rising through the acid, condenses in the upper part of the tube.

It is not acted upon by oxygen at temperatures under a red heat. A mixture of oxygen and the vapour of the substance would not inflame by a strong electric spark, though the temperature was raised by a spirit-lamp to about 400°. When oxygen mixed with the vapour of the substance is passed through a red-hot tube, there is decomposition, and mixtures of chlorine, carbonic oxide, carbonic acid, and phosgene gases are produced. A portion of the chloride was heated with peroxide of mercury in a glass tube over mercury; as soon as the oxide had given off oxygen, and the heat had risen so high as to soften the glass considerably, the vapour suddenly detonated with the oxygen with bright inflammation. The substances remaining were oxygen, carbonic acid, and calomel; and I believe there was no decomposition or action, until so much mercury had risen in vapour as to aid the oxygen by a kind of double affinity in decomposing the chloride of carbon.

Chlorine produces no change on the substance, either by exposure to light or heat.

When iodine is heated with it at low temperatures, the two substances melt and unite, and there is no farther action. When heated more strongly in vapour, the iodine separates chlorine, reducing the perchloride to the fluid protochloride of carbon, and chloriodine is produced. This dissolves, and if no excess of iodine be present, the whole remains fluid at common temperatures. When water is added, it generally liberates a little iodine; and on heating the solution, so as to

drive off all free iodine, and testing by nitrate of silver, chloride and iodide of silver are obtained.

Hydrogen and the vapour of the substance would not inflame at the temperature of 400° F. by strong electrical sparks; but when the mixture was sent through a red hot tube, the chloride was decomposed, and muriatic acid gas and charcoal produced.

The vapour of the perchloride of carbon readily detonates by the electric spark with a mixture of oxygen and hydrogen gases; but the gaseous results are very mixed and uncertain, from the near equipoise of affinities that exist among the elements.

Sulphur readily unites to it when melted with it, and the mixture crystallizes on cooling into a yellowish mass. When heated more strongly, the substance rises unchanged, and leaves the sulphur unaltered; but when the mixed vapours are raised to a still higher temperature, chloride of sulphur and proto-chloride of carbon are formed. Sometimes there are appearances as if a carburet of sulphur were formed, but of this I have not satisfied myself.

Phosphorus at low temperatures melts and unites with the substance, without any decomposition. If heated in the vapour of the substance, but not too highly, it takes away chlorine, and forms the proto-chlorides of phosphorus and carbon. If heated more highly, it frequently inflames in the vapour with a brilliant combustion, and abundance of charcoal is deposited. Sometimes I have had the charcoal left in films stretching across the tubes, and occupying the space where the flame passed. The appearance is then very beautiful.

When phosphorus is heated with the vapour of the substance over mercury, so as not to inflame in it, there is generally a small portion of muriatic acid gas formed. If great care be taken, this is in very minute quantity ; and its variable proportion sufficiently shows, that the hydrogen which forms it does not come from the substance. I am induced to believe that it is derived from moisture adhering to the phosphorus. The action of iodine on phosphorus shows, that it is very difficult to dry the latter substance perfectly.

A stick of phosphorus put into the alcoholic or etherial solution of the perchloride did not exert any action upon it.

Charcoal heated in the vapour of the substance appears to have no action upon it.

Most of the metals decompose it at high temperatures. Potassium burns brilliantly in the vapour, depositing charcoal, and forming chloride of potassium. Iron, zinc, tin, copper and mercury, act on it at a red heat, forming chlorides of those metals, and depositing charcoal; and when the experiments are made with pure substances, and very carefully, no other results are obtained. Some of the substance was passed over iron turnings heated in a glass tube. At the commencement of the sublimation of the chloride through the hot iron, the common air of the vessels was expelled, and received in different tubes ; but before one-third of the substance had been passed, all liberation of gas ceased, and the remainder was decomposed by the iron, without the production of any gaseous matters. The different portions of air that were thrown out being examined, the first proved to be common air, and the last carbonic oxide. This had resulted, probably, from the action of the chlorine on the lead of the glass tube.

An evident action had taken place, and the oxygen evolved, meeting with the liberated carbon, would produce the carbonic oxide. This experiment has been repeated several times with the same results.

When the perchloride of carbon is heated with metallic oxides, different results are produced according to the proportions of oxygen in the oxides. The peroxides, as of mercury, copper, lead, and tin, produce chlorides of those metals, and carbonic acid; and the protoxides, as those of zinc, lead, &c. produce also chlorides; but the gaseous products are mixtures of carbonic acid and carbonic oxide. I have frequently perceived the smell of phosgene gas on passing the chloride over oxide of zinc; and as the substance easily liberates chlorine at high temperatures, it will be readily seen how a small portion of that gas may be formed. It also happens, sometimes, that the protoxides become blackened from the deposition of charcoal.

When the vapour of the chloride is passed over lime, baryta, or strontia, heated red hot, a very vivid combustion is produced. The oxygen and the chlorine change places, and both the metals and the carbon are burnt. Chlorides are produced, carbonic acid is formed and absorbed by the undecomposed parts of the earths, and carbon is deposited. In these experiments no carbonic oxide is produced. When passed over magnesia, there is no action on the earth, but the perchloride of carbon is converted by the heat into protochloride.

In these experiments with the oxides no trace of water could be perceived.

Having thus far described the properties of the substance,

I shall now give the reasons which induce me to consider it a true chloride of carbon, and shall endeavour to assign its composition. My first object was to ascertain whether hydrogen existed in it, or not. When phosphorus is heated in it, a small quantity of muriatic acid is generally formed; but doubt arises as to the cause of its production, from the circumstance that the phosphorus, as already mentioned, may be the source of the hydrogen. When potassium is heated in the vapour of the substance, there is generally a small expansion of volume, and inflammable gas produced; but it is very difficult to cleanse potassium both from naptha and an adhering crust of moist potash; and either of these, though in extremely minute quantities, would give fallacious results.

A more unexceptionable experiment made with iron, has been already described; and the inferences from it are against the presence of hydrogen in the compound.

Some of the substance in vapour was electrized over mercury, by having many hundred sparks passed through it. Calomel was formed, and carbon deposited. A very minute bubble of gas was produced, but it was much too small to interfere with the conclusions drawn respecting the binary nature of the compound; and was probably caused by air that had adhered to the sides of the tube when the mercury was poured in.

The most perfect demonstration that the body contains no hydrogen, and indeed of its nature altogether, is obtained from the circumstances which attend its formation. When the fluid compound of chlorine and olefiant gas is acted on by chlorine and solar light in close vessels, although the whole of the chlorine disappears, yet there is no change of

volume, its place being occupied by muriatic acid gas. Hence, as muriatic acid gas is known to consist of equal volumes of chlorine and hydrogen, combined without condensation, it is evident that half the chlorine introduced into the vessel has combined with the elements of the fluid, and liberated an equal volume of hydrogen; and as, when the chloride is perfectly formed, it condenses no muriatic acid gas, a method, apparently free from all fallacy, is thus afforded of ascertaining its nature.

I have made many experiments on given volumes of chlorine and olefiant gases. A clean dry retort was fitted with a cap and stop-cock. Its capacity was 25.25 cubic inches. Being exhausted by the air-pump, it was filled with nitrogen (24.25 cubic inches being required), and being again exhausted, 5 cubic inches of olefiant gas, and 10 cubic inches of chlorine were introduced. It was then set aside for half an hour, that the fluid compound might form, and afterwards being placed again over a jar of chlorine, 19.25 cubic inches entered; so that the condensation had been as nearly as possible 10 cubic inches, or twice the volume of the olefiant gas (barometer 29.1 inches). It was now placed for the day (October 18th.) in the rays of the sun; but the weather was not very fine. In the evening the solid crystalline substance had formed in abundance, and very little fluid remained. When placed over chlorine, not the slightest change in volume had been produced. The stop-cock was now opened under mercury, and a small portion of the metal having entered, it was agitated in the retort, to absorb the chlorine; the neck of the retort was left open under the mercury all night, and the whole agitated from time to time. Next morning (barometer 29.6) the mercury

which had entered, being passed into the neck of the retort, stood at a certain mark 6 inches above the level of the mercury in the trough, occupying 1.25 cubic inches, and leaving 24 cubic inches filled by the expanded muriatic acid gas and nitrogen. These volumes corrected to the pressure of 29.1 inches, give 5.78 cubic inches for the chlorine absorbed, and 19.47 cubic inches for the muriatic acid gas, &c. These absorbed by water left 1.2 cubic inches of nitrogen; so that the gases in the retort, after the action of solar light, were

Muriatic acid gas -	18.27 cubic inches.
Chlorine - -	5.78
Nitrogen, &c. -	1.2

and before that action,

Chlorine - -	29.25 cubic inches.
Olefiant gas -	5.
Nitrogen - -	1.

Hence 23.47 cubic inches of chlorine had disappeared, and 9.13 of these had entered into combination with an equal volume of 9.13 cubic inches of hydrogen liberated from the 5 cubic inches of olefiant gas, to form muriatic acid; and, consequently, 14.34 cubic inches of chlorine remained combined with the carbon of the 5 cubic inches of olefiant gas. Here, the volume of chlorine actually employed is not quite five times that of the olefiant gas, nor the volume of muriatic acid gas produced, equal to four times that of the olefiant gas; but they approximate; and when it is remembered that the conversion was not quite perfect, and that the gases used would inevitably contain a slight portion of impurity, the causes of the deficiency can easily be understood.

In other experiments made in the same way, but with

smaller quantities, more accurate results were obtained. 1 cubic inch of olefiant gas with 12.25 cubic inches of chlorine, produced by the action of light 3.67 cubic inches of muriatic acid gas, 4 963 of the chlorine having been used. 1.4 cubic inch of olefiant gas with 12.5 cubic inches of chlorine produced 5.06 cubic inches of muriatic acid gas, 6.7 cubic inches of chlorine having been used. Other experiments gave very nearly the same results; and I have deduced from them, that 1 volume of olefiant gas requires 5 volumes of chlorine for its conversion into muriatic acid and chloride of carbon; that 4 volumes of muriatic acid gas are formed; that 3 volumes of chlorine combine with the 2 volumes of carbon in the olefiant gas to form the solid crystalline chloride; and that, when chlorine acts on the fluid compound of chlorine and olefiant gas, for every volume of chlorine that combines, an equal volume of hydrogen is separated.

I have endeavoured to verify these proportions by analytical experiments. The mode I adopted was, to send the substance in vapour over metals and metallic oxides at high temperatures. Considerable care is requisite in such experiments; for if the process be carried on quickly, a portion of fluid chloride of carbon is formed, and escapes decomposition. The following are two results, from a number of experiments agreeing well with each other.

5 grains were passed over peroxide of copper in an iron tube, and the gas collected over mercury; it amounted to 3.9 cubic inches, barometer 29.85; thermometer 54° F. Of these nearly 3.8 cubic inches were carbonic acid, and rather more than ,1 of a cubic inch was carbonic oxide. These are nearly equal to ,5004 of a grain of carbon. Hence, 100

of the chloride would give 10 of carbon nearly, but by calculation 100 should give 10.19. The difference is so small, as to come within the limits of errors in experiment.

5 grains were passed over peroxide of copper in a tube made of green phial glass, and the chlorine estimated in the same manner as before. 17.7 grains of chloride of silver were obtained equal to 4.36 grains of chlorine. This result approaches much nearer to the calculated result than the former; but there had still been action on the tube, and a minute portion of the substance had passed undecomposed, and condensed at the opposite end of the tube in crystals.

Experiments made by passing the perchloride over hot lime or barytes, promise to be more accurate and easy of performance. In the mean time, the above analytical results will perhaps be considered as strong corroboration of the opinion of the nature of the compound, deduced from the synthetical experiments; and the composition of the perchloride of carbon will be

3 proportions of chlorine	=	100.5
2 ditto carbon	=	11.4
		111.9

Proto-chloride of carbon.

Having said so much on the nature of the perchloride of carbon, I shall have less occasion to dwell on the proofs that the compound I am about to describe, is also a binary combination of carbon and chlorine.

When the vapour of the perchloride of carbon is heated to dull redness, chlorine is liberated, and a new compound of that element and carbon is produced. This is readily shown

by heating the bottom of a small glass tube, containing some of the perchloride, in a spirit lamp. The substance at first sublimes, but as the vapour becomes heated below, it is gradually converted into proto-chloride, and chlorine is evolved.

It is not without considerable precaution that the proto-chloride of carbon can be obtained pure; for though passed through a great length of heated tube, part of the perchloride frequently escapes decomposition. The process I have adopted is the following: some of the perchloride is introduced into the closed end of a tube, and the space above it, for ten or twelve inches, filled with small fragments of rock crystal; the part of the tube beyond this is then bent up and down two or three times, so that the angles may form receivers for the new compound; then heating the tube and crystal to bright redness, and dipping the angles in water, the perchloride is slowly sublimed by a spirit lamp, and, on passing into the hot part of the tube, is decomposed; a fluid passes over, which is condensed in the angles of the tube, and chlorine is evolved: part of the gas escapes, but the greater portion is retained in solution by the fluid, and renders it yellow. Having proceeded thus far, by the careful application of a lamp and blow-pipe, the bent part of the tube may be separated from that within the furnace, and the end closed, so as to form a small retort; and on distilling the fluid four or five times from one angle to the other, all the chlorine may be driven off without any loss of the substance, and it becomes limpid and colourless. It still, however, always contains some perchloride, which has escaped decomposition, and, to separate this, I have boiled the fluid until the tube was nearly full of its vapour, and then closing

the end that still remained open, by a lamp and blow-pipe, have afterwards left the whole to cool. It is then easy by collecting all the fluid into one end of the tube, and introducing that end through a cork into a receiver, under which a very small flame is burning, to distil the whole of the fluid at a temperature very little above that of the atmosphere. The solid chloride being less volatile does not rise so soon, and the pure proto-chloride collects at the external end of the tube. To ascertain its purity, a drop may be placed on a glass plate; it will immediately evaporate, and if it contains perchloride, that substance will be left behind; otherwise, no trace will remain on the glass. The presence or absence of free chlorine may be ascertained by dissolving a little of the fluid in alcohol or ether, and testing by nitrate of silver.

The pure proto-chloride of carbon is a highly limpid fluid, and perfectly colourless. Its specific gravity is 1.5526. It is a non-conductor of electricity. I am indebted to Dr. Wollaston for the determination of the refractive power of this chloride, and for the approximation to the refractive power given of the perchloride. In the present case it is 1.4875, being very nearly that of camphor. It is not combustible except when held in a flame, as of a spirit lamp, and then it burns with a bright yellow light, much smoke, and fumes of muriatic acid.

It does not become solid at the zero of Fahrenheit's scale. When its temperature is raised under the surface of water to between 160° and 170°, it is converted into vapour, and remains in that state until the temperature is lowered. When heated more highly, as by being passed over red hot rock crystal in a glass tube, a small portion is always decomposed; nearly all the fluid may, however, be condensed again, but

it passes slightly coloured, and the tube and crystal are blackened on the surface by charcoal. I am uncertain whether this decomposition ought not to be attributed rather to the action of the glass at this high temperature than to the heat alone.

It is not soluble in water, but remains at the bottom of it in drops, for many weeks, without any action.

It is soluble in alcohol and ether, and the solutions burn with a greenish flame, evolving fumes of muriatic acid.

It is soluble in the volatile and fixed oils. The volatile oils containing it burn with the emission of fumes of muriatic acid. When the solutions of it in the fixed oils are heated, they do not blacken or evolve fumes of muriatic acid. It is therefore probable, that when this happens with the solution of the perchloride in fixed oils, it is from its conversion by the heat into proto-chloride and the liberation of chlorine.

It is not soluble in alkaline solutions, nor do they act on it in some days. Neither is it at all soluble in, or affected by, strong nitric, muriatic, or sulphuric acids.

Solutions of silver do not act on it.

Oxygen decomposes it at high temperatures, forming carbonic oxide, or acid, and liberating chlorine.

Chlorine dissolves in it in considerable quantity, but has no farther action, or only a very slow one in common day light; on exposure to solar light, a different result takes place. I have only had two days, and those in the middle of November, on which I could expose the proto-chloride of carbon in atmospheres of chlorine to solar light; and hence the conversion of the whole of the proto-chloride was not perfect; but at the end of those two days the retorts containing the

substances were lined with crystals, which, on examination under the microscope, proved to be quadrangular plates, resembling those of the perchloride of carbon. There were also some rhomboidal crystals here and there. After the formation of these crystals, there was considerable absorption in the retort; hence chlorine had combined; and the gas which remained was chlorine unmixed with any thing else, except a slight impurity. The solid body, on examination, was found to be volatile, soluble in alcohol, precipitable by water, and had the smell and other properties of perchloride of carbon. Hence, though heat in separating chlorine from the perchloride of carbon produces its decomposition, light occasions its reproduction.

It dissolves iodine very readily, and forms a brilliant red solution, similar in colour to that made by putting iodine into sulphuret of carbon, or chloric ether. It does not exert any farther action on iodine at common temperatures.

An electric spark passed through a mixture of the vapour of the chloride with hydrogen, does not cause any detonation, but when a number are passed, the decomposition is gradually effected, and muriatic acid is formed. When hydrogen and the vapour of the proto-chloride are passed through a red hot tube, there is a complete decomposition effected, muriatic acid gas being formed, and charcoal deposited. The mixed vapour and gas burn with flame as they arrive in the hot part of the tube. The vapour of the proto-chloride detonates readily by the electric spark with a mixture of oxygen and hydrogen gases, and a complete decomposition is effected. It will not detonate with the vapour of water.

Sulphur and phosphorus both dissolve in it, but exert no

decomposing action at temperatures at, or below, the boiling point of the chloride. The hot solution of sulphur becomes a solid crystalline mass by cooling. Phosphorus decomposes it at a red heat.

Its action on metals is very similar to that of the perchloride. When passed over them at a red heat, it forms chlorides, and liberates charcoal. Potassium does not act on it immediately at common temperatures; but, when heated in its vapour, burns brilliantly, and deposits charcoal.

When passed over heated metallic oxides, chlorides of the metals are formed, and carbonic oxide, or carbonic acid, according to the state of oxidation of the metal. When its vapour is transmitted over heated lime, baryta, or strontia, the same brilliant combustion is produced as with the perchloride.

Whilst engaged in analyzing this chloride of carbon, for the purpose of ascertaining the proportions of its elements, I endeavoured, at first, to find how much chlorine was liberated from a certain weight of perchloride during its conversion into proto-chloride, and for this purpose distilled the perchloride through red hot tubes into solution of nitrate of silver, receiving the gas into tubes filled with and immersed in the same solution; but I could never get accurate results in this way, from the difficulty of producing a complete decomposition, and also from the formation of chloric acid. 5 grains of perchloride distilled in this manner gave 4.3 grains of chloride of silver, which are equivalent to 1.06 grains of chlorine; but some of the chloride evidently passed undecomposed, and crystallized in the tube.

2.7 grains of the pure proto-chloride were passed over red

hot pure baryta in a glass tube: a very brilliant combustion with flame took place, chloride of barium and carbonic acid were produced, and a little charcoal deposited. When the tube was cold, the barytes was dissolved in nitric acid, and the chlorine precipitated by nitrate of silver. 9.4 grains of dry chloride of silver were obtained = 2.32 grains of chlorine.

Other experiments were made with lime, which gave results very near to this, the quantity of chloride being rather less.

3 grains of pure proto-chloride were passed over per-oxide of copper heated red hot in an iron tube, and the gas received over mercury. 3.5 cubic inches of carbonic acid gas came over mixed with ,1 of a cubic inch of common air. These 3.5 cubic inches are nearly equal to .449 of a grain of carbon.

These experiments indicate the composition of the fluid chloride of carbon to be 1 proportion of chlorine and 1 of carbon, or 33.5 of the former and 5.7 of the latter. The difference between these theoretical numbers, and the results of the experiments, is not too great to have arisen from errors in working on such small quantities of the substance.

A mixture of equal volumes of oxygen and hydrogen was made, and 2 volumes of it detonated with the vapour of the proto-chloride in excess over mercury by the electric spark. The expansion was very nearly to 4 volumes; of these, 2 were muriatic acid, and the rest pure carbonic oxide: and calomel had been formed, its presence being ascertained by potash. Hence it appears, that 1 volume of hydrogen and half a volume of oxygen had decomposed 1 proportion of the proto-chloride, forming the two volumes of muriatic

acid gas and 1 volume of carbonic oxide; and that at the intense temperature produced within the tube by the inflammation, the rest of the oxygen and the mercury had decomposed a farther portion of the substance, giving rise to the second volume of the carbonic oxide, and to the calomel.

A mixture of 2 volumes of hydrogen and 1 volume of oxygen was made, and 3 volumes of it detonated with the vapour, as before. After cooling, the expansion was to 6 volumes; 4 of which were muriatic acid, and 2 carbonic oxide. There was no action on the mercury in this experiment. Again, 5 volumes of the same mixture being detonated with the vapour of the substance, expanded to 9.75 volumes, of which 6 25 were absorbed by water and were muriatic acid, and 3.5 were carbonic oxide mixed with a very small portion of air introduced along with the fluid chloride. These experiments, I think, establish the composition of the proto-chloride of carbon, and prove that it contains 1 proportion of each of its elements.

From a consideration of the proportions of these two chlorides of carbon, it seems extremely probable that another may exist, composed of 2 proportions of chlorine combined with 1 of carbon. I have searched assiduously for such a compound, but am undecided respecting its production. When the fluid proto-chloride was exposed with chlorine to solar light, crystals were formed, as before described. The greater number of these were certainly the perchloride first mentioned in this paper; but when the retort was examined by a microscope, some rhomboidal crystals were observed here and there among those of the usual dendritic and square forms.

These may, perhaps, be the real perchloride ; but I had not time, before the season of bright sunshine passed away, to examine minutely what happens in these circumstances ; and must defer this, with many other points, till the next year brings more favourable weather.

Compound of Iodine, Carbon, and Hydrogen.

The analogy which exists between chlorine and iodine, naturally suggested the possible existence of an iodide of carbon, and the means which had succeeded with the one element, offered the best promise of success with the other.

Iodine and olefiant gas were put in various proportions into retorts, and exposed to the sun's rays. After a while, colourless crystals formed in the vessels, and a partial vacuum was produced. The gas in the vessels being then examined, was found to contain no hydriodic acid, but only pure olefiant gas. Hence, the effect had been simply to produce a compound of the iodine with the olefiant gas.

The new body formed was obtained pure by introducing a solution of potash into the retort, which dissolved all the free iodine; the substance was then collected together and dried. It is a solid white crystalline body, having a sweet taste and aromatic smell. It sinks readily in sulphuric acid of specific gravity 1.85. It is friable ; is not a conductor of electricity When heated, it first fuses, and then sublimes without any change. Its vapour condenses into crystals, which are either prismatic, or in plates. On becoming solid after fusion, it also crystallizes in needles. The crystals are transparent. When highly heated it is decomposed, and iodine evolved. It is not readily combustible; but when

held in the flame of a spirit lamp, burns, diminishing the flame, and giving off abundance of iodine, and some fumes of hydriodic acid. It is insoluble in water, or in acid and alkaline solutions. It is soluble in alcohol and ether, and may be obtained in crystals from these solutions. The alcoholic solution is of a very sweet taste, but leaves a peculiarly sharp biting sensation on the tongue.

Sulphuric acid does not dissolve it. When heated in the acid to between 300° and 400°, the compound is decomposed, apparently by the heat alone; and iodine and a gas, probably olefiant gas, are liberated. Solution of potash acts on it very slowly, even at the boiling point, but does gradually decompose it.

This substance is evidently analogous to the compound of olefiant gas and chlorine, and remarkably resembles it in the sweetness of its taste, though it differs from it in form, &c. It will with that body form a new class of compounds, and they will require names to distinguish them. The term chloric ether, applied to the compound of olefiant gas and chlorine, did not at any time convey a very definite idea, and the analogous name of iodic ether, would evidently be very improper for a solid crystalline body heavier than sulphuric acid. Mr. Brande has suggested the names of hydriodide of carbon, and hydrochloride of carbon, for these two bodies. Perhaps as their general properties range with those of the combustibles, whilst the specific nature of the compound is decided by the supporter of combustion which is in combination, the terms of hydro-carburet of chlorine, and hydro-carburet of iodine, may be considered as appropriate for them.

As yet I have not succeeded in procuring an iodide of carbon, but I intend to pursue these experiments in a brighter season of the year, and expect to obtain this compound.

VII. *An account of the comparison of various British Standards of linear measure.* By Capt. HENRY KATER, *F. R. S. &c.*

Read January 18th, 1821.

THE Commissioners appointed to consider the subject of Weights and Measures, recommended in their First Report "for the legal determination of the standard yard, that "which was employed by General ROY in the measurement "of a Base on Hounslow Heath, as a foundation for the Tri-"gonometrical operations that have been carried on by the "Ordnance throughout the country." In consequence of this determination, it became necessary to examine the standard to which the Report alludes, with the intention of subsequently deriving from it a scale of feet and inches.

On referring to the Philosophical Transactions for 1785, it may be seen in "an Account of the Measurement of a Base "on Hounslow Heath," that a brass scale, the property of General ROY (and now in the possession of HENRY BROWNE, Esq. F.R.S.), was taken to the apartments of the Royal Society, and being there, with the assistance of Mr. RAMSDEN, compared with their standard (both having remained together two days previous to the comparison), the extent of 3 feet taken from the Society's standard, and applied to General ROY's scale, was found to reach exactly to 36 inches, at the temperature of 65°.

It afterwards appears that points, at the distance of 40

inches from each other, were laid off on a large plank from General Roy's scale, the whole length being 20 feet; and by means of this plank the length of the glass rods was determined, with which the base on Hounslow Heath was measured.

In the Philosophical Transactions for 1795, it is stated, that Mr. Ramsden compared *his* brass standard with that belonging to the Royal Society, after they had remained together about 24 hours, when " they were found to be precisely of the same " length." Brass points were then inserted in the upper surface of a cast iron triangular bar of 21 feet in length, from Mr. Ramsden's standard, at the distance of 40 inches from each other, the whole length of 20 feet being laid off on those points in the temperature of 54°.

By means of this bar, the length of the hundred feet steel chain was determined with which the base on Hounslow Heath was re-measured, and was found to be only about $2\frac{3}{4}$ inches greater than the measurement with the glass rods.

The standard scale used by Mr. Ramsden in laying off the points on the iron bar, is, it seems, no longer to be found; but from the declared equality of both this and General Roy's standard with that of the Royal Society, and the near agreement of the two separate measurements of the base with the glass rods and with the steel chain, one might have been tempted to consider General Roy's scale as precisely similar to Mr. Ramsden's, and as offering the best source from which the national standard yard might be obtained.

The spirit however, of the recommendation of the Commissioners of Weights and Measures, appearing to be, that the standard yard should be derived *from the base of the*

Trigonometrical Survey, I thought it preferable to proceed a step higher, and to obtain a distance of 40 inches from the iron bar itself, which could afterwards be employed in any manner that might be found most eligible.

In order to obviate the necessity of an allowance for temperature, I caused a triangular bar of cast iron to be made, of the same dimensions as Mr. RAMSDEN's, except as to length. Gold pins were inserted near the extremities of this bar at the distance of 40 inches from each other, on which were to be drawn fine lines, comprising one sixth part of the length of the 20 feet bar.

The apparatus used for tracing the lines on the gold pins, is essentially different from that commonly employed. The cutting point is elevated by means of an inclined plane, and is then carried through a distance equal to the length of the line to be traced. On drawing back a part of the apparatus, the extremity of which acts upon the inclined plane, the point descends by its own weight until it wholly rests upon the surface of the bar; the motion being then continued, the frame and cutting point are drawn along together, without the possibility of lateral deviation; and the point describes a line, the length of which may, by a certain contrivance, be regulated at pleasure, and its strength determined by repeating the operation. This very neat and important invention is due to M. FORTIN of Paris, and was communicated to me by M. ARAGO, whose liberal mind knows no reserve on scientific subjects. I have varied the arrangement of M. FORTIN, so as to bring the cutting point under a microscope furnished with cross wires, having an adjustment, by means of which their intersection can be brought to the line traced by the

cutting point. This, I consider, to be an essential improvement, as no accidental derangement of the cutting frame can take place without its being immediately perceptible; and the apparatus may be conveniently applied to the division of straight lines or circles, in the manner I have described in the Philosophical Transactions for 1814.

The micrometer microscopes, used in the comparison of the different standards, were those employed in the determination of the length of the seconds pendulum, the description of which may be seen in the Philosophical Transactions for 1818. But as the arrangement of Mr. RAMSDEN's bar, required that the support to which the microscopes were attached should rest on its surface, some other form of the beam carrying them became necessary for this purpose.

A board was prepared of well seasoned mahogany, 36 inches long, 3 inches wide, and $\frac{3}{4}$ thick, and an edge bar of mahogany $3\frac{1}{2}$ inches wide and $1\frac{1}{4}$ thick, was firmly fixed along the middle of it lengthwise, which most effectually prevented the possibility of flexure. To the extremities of this edge bar, and projecting beyond them, the microscopes were fixed, their cross wires being about 40 inches asunder. By this arrangement, the very important advantage was ensured, that the apparatus being laid on a plain surface, such as a scale, and the microscopes adjusted to distinct vision, on placing it on another plane scale, the object glasses of the microscopes would be precisely at the same distance from this last surface as they were from that to which they were applied in the first instance, and consequently, no error could arise from parallax.

A piece of very thin brass, usually called latin brass, was

bent round the edges of the 40 inch bar, so that the upper surface of the bar was in perfect contact with the brass, the side pressure being just sufficient to prevent any change of position in the brass, unless when moved along the bar by hand. A fine line, about the eighth of an inch long, was now drawn on one of the gold pins at right angles to the bar, and a similar line was traced on the piece of brass, which was placed so as to cover the other gold pin. The intersection of the cross wires of the tracing microscope was carefully adjusted to this last line.

Mr. Ramsden's bar, upon his decease, became the property of Mr. Berge, whose successor, Mr. Worthington, kindly granted me access to it, and facilitated my examination by every assistance in his power. The bar was placed in his workshop on tressels, and its surface carefully brought into the same plane, which was ascertained by stretching a thread from end to end.

The 40 inch bar was laid near Mr. Ramsden's bar on the 12th of April, 1820, and a thermometer placed upon it. Three thermometers were also arranged at equal distances along Mr. Ramsden's bar.

On the 13th of April I commenced my examination. The intersection of the wires of the one microscope being placed on the centre of the left hand dot, the intersection of the wires of the other microscope was brought, by means of its micrometer screw, to the centre of the right hand dot, and the reading of the micrometer registered. In this manner the six intervals of Mr. Ramsden's bar were compared in succession. It may be necessary to remark, that as the microscopes invert, the readings are to be taken in a

contrary sense, the higher number indicating defect, and *vice versa.*

	Readings.	Thermometers.
1st interval.	29,5	54,0 } Ramsden's bar.
2d.	10,0	53,5 }
3d.	10,0	53,5 }
4th.	16,5	——
5th.	10,0	53,0 Forty inch bar.
6th.	19,0	
Mean	15,9	

The difference of temperature of the two bars being so small, may safely be neglected.

The micrometer microscope was now set to 15,9 divisions, and the apparatus being laid on the 40 inch bar, the intersection of the wires of the left hand microscope was brought to the middle of the line on the gold pins, and the piece of latin brass was moved along the bar, till the middle of the line drawn upon it appeared in the intersection of the wires of the micrometer microscope. The whole having been carefully examined, the micrometer microscopes were withdrawn.

The tracing microscope was next brought over the 40 inch bar, and placed so that the intersection of its wires appeared upon the middle of the line traced upon the brass; the brass was then slid away, and a line drawn with the cutting point upon the gold surface.

I had next to compare the distance thus obtained, with the mean of the six intervals on Mr. Ramsden's bar.

First comparison.

The four thermometers being at 54°, the following readings were taken.

	Readings.		
Forty inch bar.	33,5		
RAMSDEN's bar. 1st interval	54,0	Mean of RAMSDEN's bar	Div. 36,5
2d.	33,0	—— of the forty inch bar	34,2
3d.	27,0		
4th.	38,0	Forty inch bar *longer*	2,3
5th.	30,0		
6th.	37,0		
Mean	36,5		
Forty inch bar	35,0		

Second comparison. Thermometers as before.

	Readings.		
Forty inch bar.	36,0		
RAMSDEN's bar. 1st interval	54,5		Div.
2d.	31,0	Mean of RAMSDEN's bar	37,8
3d.	26,0	—— of forty inch bar	35,5
4th.	40,0		
5th.	35,5	Forty inch bar *longer*	2,3
6th.	40,0		
Mean	37,8		
Forty inch bar	35,0		

Third comparison. Thermometers as before.

		Readings.		
	Forty inch bar	35,0		
RAMSDEN'S bar.	1st interval	58,7		Div.
	2d.	30,0	Mean of RAMSDEN'S bar	39,6
	3d.	31,0	—— of forty inch bar	35,6
	4th.	44,0		
	5th.	34,0	Forty inch bar *longer*	4,0
	6th.	40,0		
	Mean	39,6		
	Forty inch bar.	36,2		

By the mean of these comparisons, it appeared that the forty inch bar was *too long* 2,9 divisions of the micrometer, or ,000124 of an inch.*

The preceding measures were taken from the middle of the lines on the gold pins; but as it was found that these lines were not quite parallel, this accidental circumstance afforded a method, of which I availed myself, to attain a greater degree of accuracy.

The deviation of the two lines was obtained by measuring the difference of the distances of their extremities, and by the mean of six comparisons was found to be 16,8 divisions.

Now, as this is the deviation due to the whole length of the lines, they will have approached each other 2,9 divisions, at about one sixth part of their length, reckoning from their most distant extremities.

* Each division of the micrometer is $\frac{1}{23363}$ of an inch.

This portion of the line being estimated, transverse lines were drawn, indicating the points from which future measurements were to be taken.

On the 14th of April I resumed my comparisons.

Conceiving that it would be preferable to ascertain the difference between some one interval and the mean of all the intervals of Mr. RAMSDEN's bar, and afterwards to compare such interval with the forty inch bar, I now directed my attention to this object.

Fourth comparison. Thermometers 52°,5.

	Readings.
1st. interval	99,0
2d.	78,0
3d.	73,0
4th.	83,0
5th.	82,0
6th.	83,0
Mean	83,0

15th April. Fifth comparison. Thermometers 56°.

	Readings.
1st. interval	107,0
2d.	81,0
3d.	76,0
4th.	89,0
5th.	77,0
6th.	87,0
Mean	86,1

Sixth comparison. Thermometers 56°.

	Readings.
1st. interval	107,0
2d.	80,0
3d.	79,0
4th.	82,0
5th.	75,0
6th.	83,5
Mean	84,4

On examining the preceding comparisons, it may be perceived that the readings of the sixth interval differ very little from those of the mean of the whole bar.

Readings of the sixth interval.	Mean readings of all the intervals.	Value of the sixth interval + or —.
37,0	36,5	— 0,5
40,0	37,8	— 2,2
40,0	39,6	— 0,4
83,0	83,0	— 0,0
87,0	86,1	— 0,9
83,5	84,4	+ 0,9
	Mean	— 0,5

The sixth interval, therefore, is too short 0,5 of a division.

This interval was now compared with the forty inch bar, the thermometers being at 57°; the microscopes were transferred from one bar to the other alternately.

	Readings of the sixth interval.	Readings of the 40 inch bar.
	81,5	85,0
	85,3	85,0
	82,7	86,0
	83,0	83,0
	83,5	83,0
	83,0	82,0
	82,5	82,6
	82,2	82,6
	82,5	81,3
	82,0	81,5
	82,7	82,5
	82,0	81,3
Mean	82,7	83,0

From this it appears, that the forty inch bar is *shorter* than the sixth interval 0,3 of a division ; and as the sixth interval was found to be shorter than the mean of all the intervals 0,5 of a division, the result of the whole is, that the forty inch bar is shorter than one sixth of RAMSDEN's bar 0,8 of a division, or ,000034 of an inch.

I may here remark, that the differences observable between the results of the various comparisons of the intervals of RAMSDEN's bar, may be attributed to the large size and imperfect state of most of the dots; those bounding the sixth interval are fortunately the least injured.

Having thus obtained the value of the standard, from which the chain used in the Trigonometrical Survey was actually laid off, I next proceeded to compare this with General ROY's and Sir GEORGE SHUCKBURGH's scales.

It may be seen in the former part of this paper, that the temperature at which the points were laid off on Mr. RAMSDEN's bar from the brass scale, was 54°; consequently, the observed lengths of the brass scales and 40 inch bar, must be reduced to this temperature. The expansion of one foot of General ROY's scale for one degree of the thermometer was found by him to be ,0001237, and of one foot of RAMSDEN's bar ,0000740 of an inch; consequently, the excess of the expansion of 40 inches of the scale, above 40 inches of the bar, for each degree above 54° will be ,0001657 of an inch; and this quantity has been used in computing the corrections for temperature.

The comparison of both scales with the bar was made at the same time; but to avoid confusion, I have given the results in separate tables. The scales and the 40 inch bar were laid together two days previously to commencing the examination.

TABLE I.

Comparisons of the distance from zero to 40 inches of General ROY's *scale with the 40 inch bar.*

Date.	Temp.	Readings.		Difference between the scale and the bar in inches.	Correction for Temperature.	Roy's scale *shorter* than the forty inch bar.
		Bar.	Roy.			
May 7	59,0	23	32,5	—,000407	—,000829	,001236
8	62,0	14	23	—,000385	—,001326	,001711
9	65,0	41,5	43,5	—,000086	—,001823	,001909
	65,2	41	40	+,000043	—,001856	,001813
	65,3	30	26	+,000171	—,001873	,001701
	65,3	30	29	+,000043	—,001873	,001829
	64,7	27	25	+,000086	—,001773	,001687
10	67,4	36,2	24	+,000522	—,002220	,001698
					Mean	,001698

By the above comparisons, General Roy's scale appears to be shorter than the 40 inch bar ,001698 ; to which adding ,000034, the quantity by which Ramsden's bar exceeds the 40 inch bar, we have ,001732 of an inch for the difference *in defect* between General Roy's scale and the standard used in the Trigonometrical Survey, with which it was supposed to be identical.

TABLE II.

Comparisons of the distance from zero to 40 inches of Sir George Shuckburgh's *scale with the 40 inch bar.*

Date.	Temp.	Readings.		Difference between the scale and the bar in inches.	Correction for Temperature.	Shuckburgh's scale *shorter* than the forty inch bar.
		Bar.	Shuck.			
May 7	59,0	23	75,2	—,002234	—,000829	,003063
9	65,0	41,5	62	—,000877	—,001823	,002700
	65,2	41	58	—,000728	—,001856	,002584
	65,3	30	49	—,000813	—,001873	,002686
	65,3	30	51	—,000899	—,001873	,002772
	64,7	27	50	—,000984	—,001773	,002757
10	67,4	36,2	41,5	—,000227	—,002220	,002447
	67,8	33	44	—,000471	—,002287	,002758
12	67,5	42	48	—,000257	—,002237	,002494
					Mean	,002696

If to the above mean ,000034 be added as before, we have ,00273 of an inch, by which the distance from zero to 40 inches of Sir George Shuckburgh's scale is shorter than one sixth part of Ramsden's bar.

The very great difference between Ramsden's bar and General Roy's scale, made me desirous of comparing this last with the Royal Society's standard, and as I was aware of the existence of other standards of considerable importance, I resolved to examine them at the same time.

The Royal Society's scale has been described by Sir George

SHUCKBURGH: it is of brass, and about the same dimensions as General ROY's scale, which is already well known. It has three parallel lines drawn upon it lengthwise. On one of the exterior lines marked E, are two dots expressing the length of the Tower yard. This is the yard which has been heretofore called, and which I shall still call, *the Royal Society's standard.* The middle line has the Exchequer yard marked upon it; and the other exterior line has dots, at precisely the same distance as those of the Royal Society's standard.

Knowing that Mr. CAREY had made for Lieutenant Colonel LAMBTON, a standard scale, which forms the basis of the Trigonometrical Survey carried on by him in India, and aware of the importance of ascertaining the value of this in parts of other known standards, I enquired of Mr. CAREY whence it was derived, and was informed that it had been copied from a scale then in the possession of ALEXANDER AUBERT Esq. and which, after his death, was purchased by Mr. JONES, of Holborn. On application being made by the Commissioners of Weights and Measures to Mr. JONES for the loan of it, their request was readily and obligingly complied with.

This scale is of plate brass, strengthened by an edge bar: it contains 61 inches, and has the name of BIRD upon it. Two dots upon two gold pins designate the yard, from which the divisions of the scale have evidently been derived. There is also a third dot, marking, I believe, the length of the French half toise. The dots indicating the yard are those I employed. I shall call this scale *Colonel* LAMBTON's *standard.*

BIRD's Parliamentary standard yard of 1758, had already been compared with Sir GEORGE SHUCKBURGH's scale by him, and recently by myself, and found to exceed it about two ten-

thousandths of an inch. In Sir George Shuckburgh's "account of experiments for determining a standard of weights and measures," he remarks, that there existed another standard yard made by Bird, in the year 1760, which did not differ more than two ten-thousandths of an inch from the standard of 1758; but he does not say whether this difference was observed to be in excess or in defect.

As it was possible that this standard yard of 1760, might coincide with 36 inches of Sir George Shuckburgh's scale, I was anxious to compare them together, and by the exertions of Davies Gilbert, Esq. M. P. the standard of 1760 was found in the custody of the House of Commons, and confided to my care. It is (as Sir George Shuckburgh observed) precisely similar in form to the standard of 1758, the yard being marked by two dots upon gold pins, which, though very large, are in tolerable preservation.

The five standard scales which I have just described, were placed together on the 15th of June, 1820, and arranged so that those of least bulk should be farthest from me during the observations, as they would be more readily affected by the proximity of the person of the observer.

As I was desirous that comparisons of such importance should not rest wholly on my own authority, I requested Dr. Wollaston to take two series of measurements, which, together with my own, are contained in the following table.

TABLE III.

Comparisons of various standards

Date. 1820.	Temperature.	Readings of the micrometer at: The Royal Society's standard.	General Roy's scale.	Sir George Shuckburgh's scale.	Bird's standard of 1760.	Col. Lambton's standard.
June 16	61,5	19	23	43	43	66
	—	6	17	41,5	41,5	58
	—	8	14	36	36	50
	—	86	92	118	118	133
	64,0	75	93	114	114	121
	—	82	91	111	106	126
	—	78	93	112	108	127
	63,5	75	93	113	110	124
17	61,5	15	24	44	40	59
	—	7	17	39	36	53
	—	1	14	35	36	45
	64,5	59	70	94	94	115
(By Dr. Wollaston) 18	64,5	30	39	64	64	76
(By ditto)	—	21	36	56	56	66
Mean of the whole - - -		40,1	51,1	72,9	71,6	87

I now returned to the forty inch iron bar and General ROY's scale, anxious to verify my former conclusion by a fresh examination. The microscopes being fixed at the proper distance, comparisons were made, which I shall detail before I state the results afforded by the preceding table.

TABLE IV.

Farther comparisons of the distance from zero to forty inches of General Roy's *scale, with the forty inch bar.*

Date.	Temp.	Readings.		Difference between the scale and the bar in inches.	Correction for Temperatnre.	Roy's scale *shorter* than the forty inch bar.
		Bar.	Roy.			
June 19	64,5	29	30	—,000043	—,001740	,001783
	65,0	28	26	+,000086	—,001823	,001737
	65,5	30	30	—,000000	—,001906	,001906
	66,7	12	5	+,000300	—,002104	,001804
	65,2	93	89	+,000171	—,001856	,001685
20	61,7	0	13	—,000556	—,001276	,001832
	—	2	14	—,000514	—,001276	,001790
	—	0	14	—,000599	—,001276	,001875
	—	1,5	15	—,000577	—,001276	,001853
	—	3	16	—,000556	—,001326	,001882
					Mean	,001815

Adding to the mean thus obtained ,000034, the excess of one sixth of Ramsden's bar above the forty inch bar, we have ,001849 of an inch for the excess of 40 inches of the standard used in the Trigonometrical Survey, above General Roy's scale, differing from the result given by the former comparisons contained in Table I. only ,000117 of an inch, a difference which may be attributed to uncertainty of temperature. The mean of both ,00179, is probably very near the truth.

I shall now proceed to give in one view, the results deduced from Table III, by comparing each standard in succession, with that used by Colonel Lambton in the survey of India.

Excess of the following standards above Colonel Lambton's standard.	On 36 inches.
Sir G. Shuckburgh's standard - - -	+,000642
Bird's standard of 1760, - - - -	+,000659
General Roy's scale - - - - -	+,001537
Royal Society's standard - - - -	+,002007
Ramsden's bar (used in the Trigonometrical Survey of Great Britain) - - -	+,003147

If the results of the two series of comparisons made by Dr. WOLLASTON be examined, it will be seen that the greatest difference from those above given, is not two ten-thousandths of an inch, and this difference appears to have arisen almost wholly from the ill defined dots of the Royal Society's standard.

The standard used in the Trigonometrical Survey, being thus unexpectedly found to differ so considerably from every other standard of authority, the Commissioners of Weights and Measures proposed, in their Second Report, that BIRD's Parliamentary standard of 1760, should be considered as the foundation of all legal weights and measures.

It may be seen, that the standard thus selected, differs so little, if at all, from that of Sir GEORGE SHUCKBURGH, that they may, for every purpose, be considered as perfectly identical; and this agreement is particularly convenient, because the length of the mêtre having been determined by comparisons with Sir GEORGE SHUCKBURGH's scale, and a fac simile of it made by Mr. TROUGHTON, for Professor PICTET, all measures on the Continent are converted into English measure, by a reference to Sir GEORGE SHUCKBURGH's standard.

In determining the figure of the earth, by means of the measurement of distant portions of the same meridian, many anomalies have been remarked, which may, in some instances, be attributed to the difference of the standards employed in such measurements. As an example of the importance of this consideration, I shall examine the results deduced by Lieutenant Colonel LAMBTON, from a comparison of three sections of the great arc measured by him in India, with the lengths of the French, the English, and the Swedish degrees. The

abridgement of Lieutenant Colonel LAMBTON's very important operations, may be found in the Philosophical Transactions for 1818.

The following are the data given by Colonel LAMBTON.

The length of the degree due to

Lat. 9°	$34'$	$44''$	is	60472,83 fathoms.
13	2	55		60487,56
16	34	42		60512,78

By the French measurement, in

Lat. 47	30	46	60779 fathoms

By the English, in

Lat. 52	2	20	60820

By the Swedish, in

Lat. 66	20	12	60955

and by successively comparing the lengths of the European degrees with the three sections of the Indian arc, Colonel LAMBTON obtains for the compression

By the French $\frac{1}{305,73}$ $\frac{1}{306,7}$ $\frac{1}{315,03}$ mean $\frac{1}{309,15}$

By the English $\frac{1}{310,28}$ $\frac{1}{311,36}$ $\frac{1}{318,97}$ mean $\frac{1}{313,54}$

By the Swedish $\frac{1}{305,14}$ $\frac{1}{305,72}$ $\frac{1}{310,72}$ mean $\frac{1}{307,19}$

and the mean of the three means $= \frac{1}{309,96}$.

In order to reduce the preceding measurements to the English national standard, we have to multiply the fathoms of the Indian degree by $-,000018$, and of the English by $+,00007$, to obtain the correction to be applied, with its proper sign, to the length of the degree. The French and Swedish degrees require no correction.

We have then the following data for computation.

By sections of the Indian arc	9° 34′ 44″	60471,74 fathoms
	12 2 55	60486,47
	16 34 42	60511,69
The French	47 30 46	60779
English	52 2 20	60824,25
Swedish	66 20 12	60955

and the resulting compression,

By the French $\frac{1}{304,64}$ $\frac{1}{305,55}$ $\frac{1}{313,77}$ mean $\frac{1}{307,99}$

English $\frac{1}{305,57}$ $\frac{1}{306,40}$ $\frac{1}{313,50}$ mean $\frac{1}{308,49}$

Swedish $\frac{}{304,44}$ $\frac{1}{305,01}$ $\frac{1}{309,09}$ mean $\frac{1}{307,55}$

and the mean of the three means $= \frac{1}{307,55}$

As it appears that the compression obtained by employing the length of the degree in Lat. 16° 34′ 42″ is uniformly in defect, whilst the results deduced from the other two sections are very nearly alike, it might perhaps be allowable to consider $\frac{1}{305,32}$, the mean of these last results, as the true compression; and this would agree very nearly with the deduction of M. LAPLACE, from the lunar irregularities; with the result of Dr. YOUNG's interesting and novel investigation, by a comparison of the mean, with the superficial density of the earth; and with the conjecture I have hazarded from the compression given by the experiments on the length of the pendulum at Unst and Portsoy.

3rd August, 1820.

VIII. *An Account of the urinary organs and urine of two species of the genus Rana.* *By* JOHN DAVY, *M. D. F. R. S.*

Read January 18, 1821.

IN a letter to Sir HUMPHRY DAVY, which was written almost two years ago, and which I understand has been honoured with a place in the Transactions of the Royal Society, I have described the kidneys of many different animals of the class Amphibia, and have shown, with one exception not there insisted on, that their urinary secretion is very similar, consisting almost entirely of uric acid.

The exception which occurred to me, was in the case of frogs, on whose urinary organs and secretion I have had an opportunity, lately, of making fresh and more minute enquiry, the results of which I beg leave to communicate.

I shall first relate the observations I have collected on the urinary organs of these animals, and then describe the experiments I have made to determine the nature of their urine.

The examination I have engaged in, has been limited to two species, the bull-frog (Rana taurina, Cuv.) and the brown-toad (Bufo fuscus, Laurenti) which are both very common in this neighbourhood; the former inhabiting the lake of Colombo, where it occasionally grows to a great size; and the latter frequenting houses, and abounding by night in the streets of the Pettah.

The kidneys of the bull-frog are apart, one on each side of

the spine; comparatively pretty large; very much lobulated; of a bright red colour, and rather tender.

The ureters do not terminate in the bladder, but in the rectum, by two soft papillæ projecting a little, and situated between the orifice of the bladder and the anus, nearer the former than the latter.

The bladder of urine is of large dimensions; nearly globular; semi-transparent, and yet pretty strong and contractile. It opens into the rectum, a few lines behind the anus, by a large orifice, very well adapted to receive the urine, as it flows from the ureters, when the anus is closed, as it usually is, by its powerful sphincter muscle.

The urinary organs of the brown-toad resemble, in most respects, those of the green-frog. In two specimens, out of many that I have dissected, I have found the kidneys incorporated at their upper ends. The ureters have the same termination nearly. The bladder of urine appears to be double; when distended fully with air, it resembles two oval bags; the compartments communicate freely just above the symphysis pubis, to which they are firmly attached; and they have but one orifice into the rectum, which is as well suited as in the former instance, for the reception of the urine as it flows into the rectum.

The urine of the bull-frog, taken from the bladder immediately after the death of the animal, varies a little in its appearance in different instances; and, of course varies considerably in quantity, the bladder being sometimes full almost to distension, and at other times quite empty. The following is a description of a quantity of urine amounting to 300 grains which was collected from thirty-six frogs of different sizes:

It looked like water, and was almost transparent. It was insipid, but not without smell; it emitted an odour not unlike that of serum of blood. It was of sp. grav. 1003.

It is obvious, that with its appearance its chemical nature must also vary. The urine, the physical properties of which I have described, had no effect on litmus or turmeric paper; slowly evaporated, it afforded a minute quantity of brownish extract, which had the smell of urea. It deliquesces when exposed to the air; and when decomposed by heat in a small glass tube, it yielded a little amber-coloured oily fluid and strong ammoniacal fumes; and a coal remained, in which I discovered a large proportion of common salt and a little phosphate of lime.

Another specimen of the urine of these frogs, which I examined, was rather more dilute. I detected in it a minute portion of common salt and of phosphate of lime, without any traces of urea.

The urine of the brown-toad is pretty uniform in different instances in its appearance; and, judging from the experiments I have made, in its nature also. From eighty-four toads, caught in the streets of the Pettah, 732 grains of urine were collected. Examined when quite fresh, it was nearly transparent, and would have been perfectly so, but for a few minute flocculi suspended in it. It was of a pretty bright straw yellow, very like healthy human urine in appearance, with the peculiar smell of human urine, and nearly the same taste in a slight degree. It was of sp. grav. 1008.

It did not alter litmus or turmeric paper. Nitrate of silver dropt into it, produced a very copious precipitate of luna cornea. A solution of corrosive sublimate occasioned a minute

flocculent precipitate. Neutral acetate of lead, a copious white precipitate. *Aqua ammoniæ* had no effect. Oxalate of ammonia produced a slight cloudiness; and a faint cloudiness was produced by muriate of barytes, which did not disappear on the addition of a drop of nitric acid. A portion of this urine, slowly evaporated, afforded a brown extract, with a strong urinous smell. To a moiety of this extract of a syrupy consistence, a drop of nitric acid was added; the effect produced was just the same as if human urine had been the subject of the experiment; a crystalline compound was immediately formed, which I could not hesitate in pronouncing nitrate of urea. The other moiety, decomposed by heat in a close glass tube, afforded a considerable quantity of yellow oily fluid, strongly impregnated with subcarbonate of ammonia, and a residual coal, from which I obtained a large proportion of common salt and a little phosphate of lime, and slight traces of a fixed alkaline phosphate.

Another portion of this urine was set aside to undergo spontaneous decomposition. It has been kept now eight days. It has become slightly turbid, and has acquired a distinct, though not strong ammoniacal odour, mixed with another kind of odour, not unlike that of cabbage.

The conclusions to be drawn from the results of these experiments scarcely need to be pointed out: it is pretty evident, now, that the urine of the bull-frog and of the brown-toad contains urea, and the latter rather abundantly. Reasoning from analogy, the probability is, that the urine of frogs and toads in general is of a similar nature, and altogether different from that of the other amphibia.

It is seldom that any very abrupt transitions are to be

observed in nature: the urinary organs of the turtle and tortoise, seem to be a connecting link between those of the animals in question, and those of serpents and lizards.

Perhaps additional facts are not required to prove, that the secretion of the kidneys of animals depends more on the intimate and invisible structure of these organs, than on the kind of food the animals consume; were such facts wanting, there would be no difficulty in furnishing them. How different is the urine of the brown-toad and that of any species of small lizards! yet flies are the favourite and common diet of both animals. Other remarkable instances might be mentioned, of similarity of diet and difference of urinary secretion; and, *vice versa*, instances might be afforded of difference of diet and similarity of urine: I will mention one only; it is that of parrots and snakes; their urine, as I have found, being much the same, consisting chiefly of uric acid, though their diet is altogether different, the birds feeding entirely on vegetable matter, and the reptiles entirely on animal matter. But let me not be supposed to maintain that the urinary secretion depends entirely on the organ, quite independent of the nature of the food or of the blood, from which the elements of the urine are derived. It appears to be pretty satisfactorily proved, that, *cæteris paribus*, there is a certain relation between the nature of the food and of the urine. Whilst this has been generally admitted, the relation between the organ and the secretion has been less insisted on, though perhaps not less curious and deserving of attention.

I have hitherto made no allusion to the difference of opinion amongst comparative anatomists, on the subject of the urinary organs of the frog, whether it has, or has not, an urinary

bladder; nor do I propose now, more than barely to allude to it. I flatter myself, that the analysis I have given of the contents of the bladder of the frog and toad, not to mention the anatomical observations detailed, will satisfy the sceptical, that the bladder of these animals is really a bladder of urine, according to the opinion long since advanced by M. CUVIER.

Colombo, Ceylon, January 28, 1819.

IX. *An account of a Micrometer made of Rock Crystal. By* G. Dollond, *F. R. S.*

Read January 25, 1821.

Rock crystal having been applied to telescopes in various ways, for the purposes of micrometrical measurements, particularly that which is recommended by M. Arago, induced me to consider if a more simple mode of applying the crystal could not be discovered; and the following account of its application to the eye tube of a telescope, is the result.

The improvement consists in making a sphere or lens from a piece of rock crystal, and adapting it to a telescope in the place of the usual eye-glass; and from its natural double refracting property, rendering it useful as a micrometer.

The advantages of thus applying the crystal are, in the first place, the very great saving of the time required to find the proper angle for cutting the crystal; also of cutting the prisms to their proper angles, and working their surfaces with sufficient accuracy to render them useful as micrometers, in the manner that is recommended by M. Arago, Dr. Wollaston, and others.

Upon the plan which is now submitted, it is only necessary to select a piece of perfect crystal; and without any knowledge of the angle that will give the greatest double refraction, to form the sphere of a proper diameter for the focal length required.

The second advantage is derived from being able to take the angle on each side zero, without reversing the eye tube;

also of taking intermediate angles between zero and the greatest separation of the images, without exchanging any part of the eye tube, it being only required to move the axis in which the sphere is placed.

Thirdly, it possesses the property of an eye tube or lens that is not intended for micrometrical measurements; for when the axis of the crystal is parallel to the axis of the object-glass of the telescope, only one image will be formed, and that will be as distinctly formed as with any lens that does not possess the double refracting property.

The eye tube is so constructed, that the plane through which the two images move, can be placed parallel to the line in the object which is to be measured; and if this motion is furnished with a divided circle, it will correctly answer the purpose of a position micrometer.

The value of the scale is found from the known diameter of any distant object, and will vary in proportion to the magnifying powers of the eye tube; its value increasing in proportion to the increase of those magnifying powers.

The preceding remarks appearing to be sufficient to elucidate the novelty of the application, I shall now endeavour to render the contrivance more explicit by references to the plate. [See Pl. IX.]

Fig. 1. Is a section of the eye tube; and Fig. 2. a general view of the same; both of the full size.

The sphere or lens, *a*, fig. 1. is formed of rock crystal, and placed in half holes, from which is extended the axis *b*, *b*, with an index attached; which index registers the motion of the sphere, the extent of that motion being shown upon the divided face *c*, fig. 2. The sphere is so placed in the half

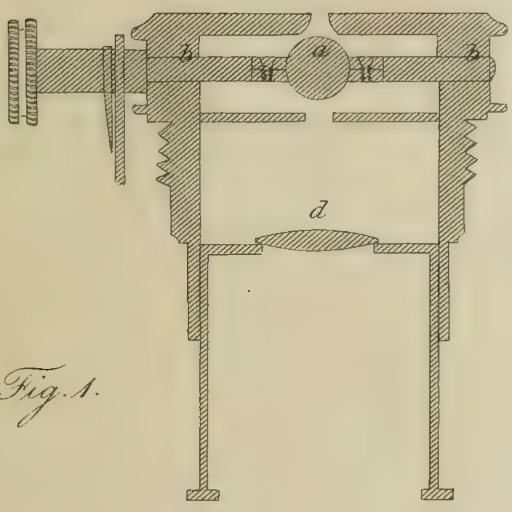


Fig. 1.

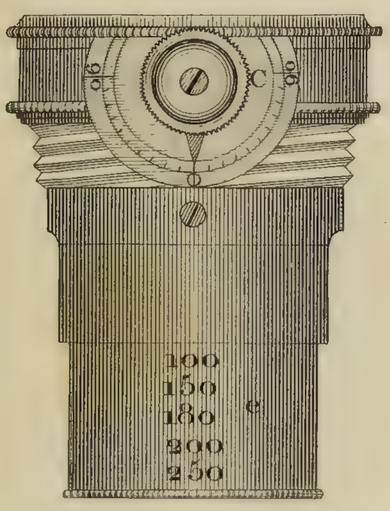


Fig. 2.

Js. Basire sc.

holes, that when its natural axis is parallel to the axis of the telescope, only one image of the object is seen. In the other direction, or that which is at right angles to the axis of motion, it must be so placed, that when it is moved, the separation of the images, viz. the ordinary and extraordinary, may be parallel to that motion. The method of acquiring this adjustment is, by turning the sphere in the half holes parallel to its own axis.

The field of view of the eye tube is increased, and the magnifying power varied, by the introduction of the lens *d*, fig. 1, between the sphere and the primary image of the object-glass; and its distance from the sphere will be in proportion to the magnifying power required; the magnifying powers are engraved upon the eye tube at *e*, fig. 2, and will vary in proportion to the focal length of the object-glass to which the eye tube is applied.

Those marked in the figure, are for an object-glass of 44 inches in focal length.

When I constructed this micrometer, it was my intention to have applied it to the measurement of the angles that are subtended by the apparent diameters of the fixed stars, as seen in achromatic refracting telescopes, for the purpose of determining their relative magnitudes; also of measuring the distances of those double stars that would come within the range of the micrometer; but from being compelled to attend to business of more immediate consequence, I am not able to accompany this description with any measurements that are sufficiently important to be interesting; although I am fully convinced from the trials I have made, that the micrometer is quite equal to the purposes for which it was intended.

X. *The Bakerian Lecture. On the best kind of steel and form for a compass needle. By Capt.* Henry Kater, *F. R. S.*

Read February 1, 1821.

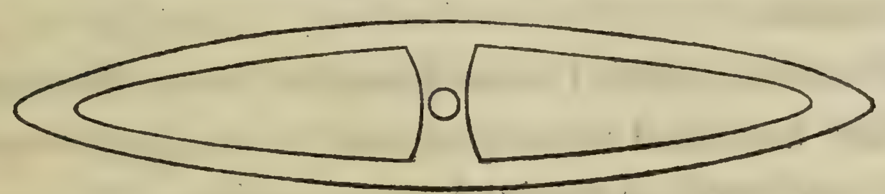
On the return of the first expedition which sailed for the discovery of a north-west passage, it appeared that from the near approach to the magnetic pole, and the consequent diminution of the directive force, the compasses on board had become nearly useless. Some of the azimuth compasses employed on that occasion were of my own invention; I was therefore anxious that the next expedition, which was about to sail under the command of Lieutenant Parry, and which has happily returned with so much honour to those engaged in it, should be furnished with instruments of this description, combining as much power and sensibility as possible.

It was with this intention alone that I commenced the experiments which form the subject of the present paper; but which I should not have deemed sufficiently important to be made public, had I not lately, on resuming the enquiry, been led to some results which appeared of sufficient interest, as well as practical utility, to induce me to lay them before the Royal Society.

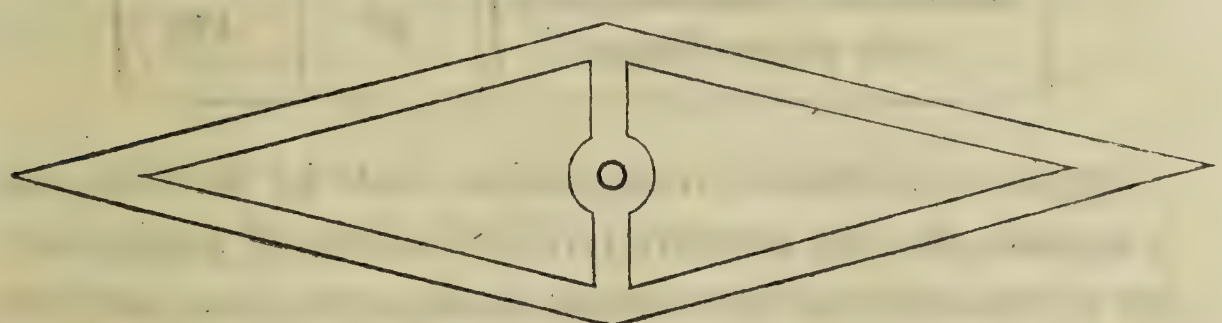
My immediate object was to ascertain the kind of steel, and form of needle best calculated to receive the greatest directive energy with the least weight.

Two needles were prepared of that kind of steel which is called blister steel, and two of spur steel, the weight of each

being 66 grains. They were of the form of a long ellipse, in length 5 inches, and in width half an inch. One of each kind was pierced, as in the figure below, the weight being made up by additional thickness. This needle, therefore, had much less extent of surface than the solid ellipse.



Recollecting to have had in my possession, many years since, a compass of extraordinary power, the needle of which was composed of pieces of steel wire put together in the shape of a rhombus, I caused two needles to be made of this form of a piece of clock-spring, which I understand is of that kind of steel which is called shear steel. They were shaped as below; in one the cross piece was of brass, and in the other formed of part of the clock spring. These needles were, by mistake, made to weigh only 45 grains.



In ascertaining the directive force, the balance of torsion of M. Coulomb was employed. This instrument, as is well known, consists of a fine wire attached to an index moveable round a circle, divided into degrees. To the other end of the wire is fixed a cradle, to receive the needle which is the sub-

ject of experiment. The needle being in the magnetic meridian when the wire has no torsion, is afterwards forced to deviate from it to a mark distant about 60°, by turning the index, and consequently twisting the wire. The number of degrees passed over by the index will be as the directive force of the needle.

The needles which I have described were first made soft, and then hardened merely at their ends; they were not polished, and were magnetized to saturation.

Experiment 1.

Needles soft, and then hardened at the ends	Weight of needle.	Directive force.
Blister steel, solid ellipse	66	500
———, open ellipse	66	520
Spur steel, solid ellipse	66	540
———, open ellipse	66	500
Shear steel, rhombus	45	435
———, rhombus, with cross piece of brass	45	435

By the experiments on magnetism made by M. COULOMB, it appears, that the directive forces of needles of similar form are to each other as their masses; the directive force, therefore, of a needle of the form of a pierced rhombus of 66 grains, would be expressed, according to the preceding experiments, by 638.

From many other experiments, which I regret were not registered at the time, it appeared that shear steel was capable of receiving the greater magnetic force, and that the

pierced rhombus was the best form for a compass needle. I may add, that needles of cast steel were tried, but were found so very inferior as to be at once rejected.

My next object was to determine the effect of polish, and of various modes of hardening and tempering the needles. In addition to the former needles, two were made of *clock-spring* of the pierced rhombus form, 5 inches long, 2 inches wide, and weighing 66 grains. One of these was first softened, then hardened at the ends, and left unpolished; the other, as well as the solid elliptical needle of spur steel, was hardened throughout, and polished. The needles were then magnetized to saturation.

Experiment 2.

	Directive force.
Unpolished rhombus, hard at the ends	800
Polished rhombus, hard throughout -	367
Polished elliptical needle, hard throughout	380
Polished elliptical needle, softened in the middle by laying it on a red hot poker	610
Polished rhombus, softened in the middle in the same manner	590
The needles were now laid aside till the following day, when the directive force was again examined.	
Unpolished rhombus, hard at the ends -	805
Polished elliptical needle, softened in the middle - - - -	625
Polished rhombus, softened in the middle	580

The polished rhombus was now softened throughout, and the extremities being hardened at a red heat, the directive force was found to be 800. It is scarcely necessary to say, that the needles were re-magnetized to saturation previous to each experiment.

From these experiments I drew the following conclusions.

That of the steel I employed, shear steel is the best kind for compass needles.

That the best form for a compass needle is that of a pierced rhombus.

That polish has no influence on the directive force.

That hardening the needle throughout, considerably diminishes its capacity for magnetism.

That a needle soft in the middle, and its extremities hardened at a red heat, appears to be susceptible of the greatest directive force.

That the directive force does not depend on the extent of surface, but on the mass.

I might also have inferred, that the needle was capable of a greater directive force when wholly softened and hardened at the extremities, than when entirely hardened and softened in the middle; but it will appear by subsequent experiments, to be detailed, that the difference is probably to be attributed to a difference in the degree of heat to which the needle is exposed in softening it in the middle.

My next experiments were made with three needles, two of which were rectangular parallelograms of equal length and weight, but the one only half the width of the other. The third needle was a pierced rhombus; the whole were made of

clock spring. These needles were made perfectly hard, and magnetized, as was always the case, to saturation.

Experiment 9.

Needles perfectly hard.	Directive force.
Wide parallelogram -	490
Narrow parallelogram	490
Pierced rhombus - -	532

An accident happened to these needles, which rendered them unfit for farther experiment. It however appears from that above stated, that the directive force is nearly as the mass, and not as the surface; and that the pierced rhombus is superior to the parallelogram.

M. Coulomb having found that a needle of the rhombus form not pierced, and which he calls " une lame taillée en flêche", was susceptible of a greater directive force than a parallelogram, I was desirous of repeating this experiment, as well as of comparing this form with the pierced rhombus. For this purpose four needles were made four inches and a half long, each weighing 63 grains; one was a parallelogram, 0,44 inch wide; another a rhombus, which I shall call the large rhombus, 0,8 wide; the third a pierced rhombus, 1,4 wide in the middle, having its sides 0,2 wide: these were made of clock-spring. The fourth needle a rhombus, which may be called the small rhombus, 0,4 wide, was made of that kind of steel which is used for saw blades, and which I believe is shear steel. This last needle was much thicker than the others.

The steel of which these needles were made had been exposed to a sufficient degree of heat to render it soft enough to be worked, and in this state the needles were magnetized.

Experiment 4.

Steel soft as worked.	Directive force.
Parallelogram - -	720
Small rhombus -	530
Large rhombus - -	765
Pierced rhombus - -	813

Experiment 5.

The ends of the needles hardened at an obscure red heat.	Directive force.
Parallelogram - -	715
Small rhombus - -	577
Large rhombus -	790
Pierced rhombus - -	840

Experiment 6.

The ends hardened at a red heat.	Directive force.
Parallelogram - -	742
Small rhombus - -	583
Large rhombus -	745
Pierced rhombus - -	844

Experiment 7.

Hardened with a bright red heat, and then softened by a red heat from the middle towards the ends, the extremities for about an inch remaining hard.	Directive force.
Parallelogram - -	611
Small rhombus - -	710
Large rhombus - -	660
Pierced rhombus - -	685

Experiment 8.

Softened at a red heat between two plates of steel, the whole being allowed to cool gradually, and then the extremities of the needles hardened at a red heat.	Directive force.
Parallelogram - -	520
Small rhombus - -	585
Large rhombus - -	554
Pierced rhombus - -	590

As it appeared from the above experiments that the needles had suffered a gradual deterioration, I imagined that this might have occurred in consequence of their having been exposed to the heat of a coal fire, by which some portion of the carbon of the steel might have been destroyed; I therefore re-carbonized the needles, by surrounding them with shreds of leather, and exposing them for several hours in a close vessel to a considerable heat. After they had gradually cooled, the ends were hardened at a red heat.

Experiment 9.

Needles soft, and then the ends hardened at a red heat.	Directive force.
Small rhombus - -	477
Large rhombus - -	415
Pierced rhombus -	450

Here we may remark, that though the needles were apparently in the same state, except being re-carbonized, as in the last experiment, they had suffered considerable deterioration.

The needles were now covered with a mixture well known to workmen to prevent decarbonization : this had been before neglected, but was used in all the subsequent experiments. They were hardened at a bright red heat, and afterwards tempered throughout rather beyond a blue colour. The large rhombus and the parallelogram were accidentally broken.

Experiment 10

Hardened at a bright red, and then tempered beyond a blue.	Directive force.
Small rhombus - -	660
Pierced rhombus -	577

From these last experiments I believe little can be gathered, except that the needles became less susceptible of directive force from repeated exposure to heat, and that this effect was not occasioned by a decarbonization of the steel. The small rhombus of saw blade, perhaps, from being the thickest, suffered less than those made of clock-spring.

The springs of clocks are made by passing the steel between rollers; and it thus undergoes great compression. May not this state be favourable to magnetism; and the repeated expansion of the steel by heat, destroying this state, have occasioned the deterioration I have remarked?

The needle which was made of saw blade having suffered less than the others in the preceding experiments, I procured three other needles of this material; they were cut out of the same plate; the weight of each was 120 grains, and their length four inches and a half. One was a parallelogram, 0,46 inch wide; another a rhombus, as before, 0,87 inch wide; and the third a pierced rhombus, having the middle 1,5 inch, and its sides 0,25 wide.

These needles were made without its being found necessary to soften the steel plate; they consequently were all as nearly as possible of the same degree of temper. In this state they were magnetized.

Experiment 11.

Steel the same as worked.	Directive force.
Parallelogram -	1143
Rhombus - -	1020
Pierced rhombus -	1085

Wishing to try whether the needles were magnetized to saturation, I carefully re-magnetized them.

Experiment 12.

Needles re-magnetized,	Directive force.
Parallelogram - -	1140
Rhombus - -	955
Pierced rhombus -	1069

I now to my surprise found that the directive force, instead of increasing, had lessened in each of the needles, and I became anxious to discover the cause of so unexpected a phenomenon. It has been observed by M. COULOMB, and more fully entered into by BIOT, that if a needle be magnetized to saturation by strong magnets, and afterwards weaker magnets be applied, the needle will lose some part of the force it had before acquired. Now, if in using the same set of magnets a certain degree of force be communicated to a needle, and the magnets be afterwards arranged in a manner less favourable for imparting magnetic force, it should seem, that this second operation would produce the same effect as would follow the use of magnets of less force, and that the magnetism of the needle would suffer a diminution.

The method I had employed in magnetizing the needles, was that of DU HAMEL, by joining the opposite poles of the magnets, and placing them on the centre of the needle, so inclined that each formed an angle, as I afterwards ascertained, of about 30 degrees with the horizon. The magnets were then slid from the centre to the extremities of the needle, and their poles being again joined at a distance from the needle, the operation was repeated.

As I could in no way account for the diminution of directive

force which I have remarked, except by supposing that I had inadvertently changed the inclination at which the magnets were held, I resolved to try whether a variation of this angle produced any considerable difference in the degree of magnetism communicated. For this purpose I re-magnetized the needles, by laying the magnets, with their opposite poles joined, flat upon the needle, the junction of the magnets being upon the centre. They were then separated and drawn to the extremities of the needle, the surface of the needle and that of the magnets being in contact the whole time. The poles were then joined and the operation repeated, using but little pressure.

Experiment 13.

Magnets moved flat upon the needle with little pressure.	Directive force.
Parallelogram - -	1265
Rhombus - -	1048
Pierced rhombus -	1130

This manner of magnetizing, therefore, appears much superior to that before employed.

The needles were again magnetized in the same manner as in the last experiment, except that the ends of the magnets were pressed pretty strongly against the needle.

Experiment 14.

Magnets moved as before, but with strong pressure.	Directive force.
Parallelogram - -	1263
Rhombus - -	1005
Pierced rhombus -	1131

No advantage appears to have resulted from the increased pressure, but the arrow-shaped needle has suffered a diminution of power

The magnets were now slid from the middle to the extremities of the needle at an inclination of only two or three degrees, and the following were the results.

Experiment 15.

Magnets inclined in an angle of two or three degrees.	Directive force.
Parallelogram - -	1275
Rhombus - -	1051
Pierced rhombus - -	1150

This method appears to be preferable to any I have yet tried, and was therefore employed in the subsequent experiments of the present series. The rhombus of 63 grains, which in Experiment 10 was left with a directive force of 577, on being magnetized in this manner had its power increased to 600.

The ends of the needles were now hardened at a red heat, the middle remaining soft, as before.

Experiment 16.

Needles hardened at the ends at a red heat.	Directive force.
Parallelogram - -	1315
Rhombus - -	1020
Pierced rhombus - -	1185

Experiment 17.

Ends hardened at a bright red.	Directive force.
Parallelogram - -	1258
Rhombus - -	970
Pierced rhombus - -	1085

The ends hardened at a red heat as near to that employed in Experiment 16, as possible.

Experiment 18.

Ends hardened at a red heat.	Directive force.
Parallelogram -	1350
Rhombus - -	1121
Pierced rhombus -	1205

Here it should seem that an increase of power has been

obtained by the ends of the needles having been first hardened at a higher temperature, and then at a lower.

The needles were now hardened throughout at a bright red heat.

Experiment 19.

Hardened throughout at a bright red.	Directive force.
Parallelogram - -	1120
Rhombus - -	1205
Pierced rhombus - -	1080

The needles softened by laying them on a red hot poker till they passed beyond the blue to a greyish white. This was carried to within an inch of their extremities, which remained hard.

Experiment 20.

Softened from the middle to a greyish white, ends hard.	Directive force.
Parallelogram - -	1360
Rhombus - -	1140
Pierced rhombus -	1210

The tempering was carried throughout the needles, the parallelogram was reserved for another purpose.

Experiment 21.

Softened throughout to a greyish white.	Directive force.
Rhombus - -	1075
Pierced rhombus -	1145

Experiment 22.

Softened throughout to a greyish white, the ends hardened at a red heat.	Directive force.
Rhombus - -	1025
Pierced rhombus -	1185

Experiment 23.

Hardened throughout, and then softened to a greyish white, as in Experiment 21.	Directive force.
Rhombus - -	1065
Pierced rhombus -	1180

This last series of experiments presents a curious circumstance. From the experiments made by Coulomb, as well as from the general tenor of my own, the rhombus is found capable of receiving a greater directive energy than the parallelogram; yet here we perceive that the parallelogram, though formed of the very same plate of steel as the other needles, is not only under every circumstance superior to the rhombus, but also to the pierced rhombus. It is difficult to form any plausible conjecture as to the cause of this difference.

The weight of the rhombus in Experiment 10, made of clock spring, was 63 grains; that made of saw blade weighed 120 grains, or very nearly double. The directive energy of the former, after having suffered great deterioration, and when not tempered in the most favourable manner, compared with the greatest directive energy of the latter, was as 600 to

1210; but if we refer to Experiment 6, it may be seen that the greatest directive energy of the clock spring rhombus was 844, which gives it an advantage of about one third in directive energy over a needle of equal weight made of saw blade.

From Experiment 20, it should seem that a needle is susceptible of the greatest directive power, other circumstances being similar, when it is hardened throughout at a red heat, and then softened from the middle to within an inch of the extremities, till the blue colour which arises has again disappeared.

I next proceeded to try, in a more regular manner, the effect of different methods of magnetizing, and at the same time to ascertain whether the directive force was influenced by extent of surface, independent of mass. Two needles were made of the same kind of steel, in the form of right-angled parallelograms, five inches long, the one 0,7 inch wide, and the other half this width. The widest was reduced in thickness until it was of the same weight as the other, viz. 142 grains. They were in the same state of softness as was necessary to work them. The magnets were placed together perpendicularly on the centre of the needle, their opposite poles being joined; their lower extremities were then separated and kept asunder by placing a piece of wood a quarter of an inch thick between them, their upper extremities remaining in contact. The magnets were then slid along the needle backwards and forwards from end to end: this was repeated on both sides, till it was conceived the needle must be saturated.

Experiment 24.

	Directive force.
Small parallelogram -	655
Large parallelogram -	674

The needles were again magnetized in the same manner as before, excepting that the magnets were separated at the top by a piece of wood of the same thickness as that at the bottom.

Experiment 25.

	Directive force.
Small parallelogram -	595
Large parallelogram -	580

The magnets were placed perpendicularly together on the centre of the needle, and then their lower extremities separated by a piece of wood to the distance of half the length of the needle, the upper extremities remaining in contact. They were then slid on the needle backwards and forwards from end to end, as before.

Experiment 26.

	Directive force.
Small parallelogram -	760
Large parallelogram -	780

The magnets joined, placed perpendicularly on the centre of the needle, as before, then moved in opposite directions from the centre to the extremities, keeping each magnet perpendicular to the needle; afterwards joined at a distance from the needle, placed again on its centre, and the operation thus continued.

Experiment 27.

	Directive force.
Small parallelogram -	993
Large parallelogram -	1155

Remarking that the surface of the small parallelogram was unequal, so as to be touched by the magnet in very few places, I filed it flat, and having reduced the large parallelogram to the same weight, they were magnetized by joining the magnets, placing them perpendicularly on the centre of the needle, separating their lower extremities, and carrying them to each end of the needle, the upper ends remaining in contact.

Experiment 28.

	Directive force.
Small parallelogram -	1025
Large parallelogram -	1150

The needles were next magnetized according to the method of DU HAMEL, the magnets being inclined at an angle

of about 45 degrees, and carried, as before, from the centre to the ends of the needle.

Experiment 29.

	Directive force.
Small parallelogram -	1070
Large parallelogram -	1170

The magnets forming with the needle an angle of about 20 degrees.

Experiment 30.

	Directive force.
Small parallelogram -	1085
Large parallelogram -	1195

Magnets forming an angle with the needle of about two or three degrees.

Experiment 31.

	Directive force.
Small parallelogram -	1160
Large parallelogram -	1275

Magnets laid flat on the surface of the needle, and drawn from the centre to the ends.

Experiment 32.

	Directive force.
Small parallelogram -	1158
Large parallelogram -	1261

Magnets forming with the needle an angle of two or three degrees, their other extremities being connected by a very soft iron wire.

Experiment 33.

	Directive force.
Small parallelogram -	1145
Large parallelogram -	1261

The iron wire was now removed and the needle magnetized, as before, at an angle of about two or three degrees.

Experiment 34.

	Directive force.
Small parallelogram -	1160
Large parallelogram -	1273

I now hardened both the needles throughout at a bright red, and then softened them from the middle to within three quarters of an inch of the ends till the blue had disappeared. This was done by laying the large parallelogram on a red

hot poker, but from the thickness of the small parallelogram this heat was found insufficient, and that of a lamp was employed. The needles were then magnetized as in the last experiment.

Experiment 35.

	Directive force.
Small parallelogram -	1815
Large parallelogram -	1660

It occurred to me that the heat employed in tempering the large parallelogram might not have been sufficient, it was therefore exposed to the flame of the lamp, but in doing this, a small piece which weighed 10 grains was broken off from its end. It was however re-magnetized, and the directive force was now found to be increased to 1720.

From these last experiments, it appears that the greatest directive force was given to the needle when the magnets were inclined to it in an angle not exceeding two or three degrees, and that this force is little, if at all, influenced by extent of surface; as I conceive the small difference in favour of the greater surface may be attributed to some difference in the quality of the steel, or its temper, both of which appear to have very considerable influence on the directive force.

Two needles, the one five, and the other eight inches long, were cut out of the same plate of steel; they were of equal weight, the short one being of greater width than the other. Being magnetized to saturation, their directive forces were as follow:

Experiment 36.

	Directive force.
Long parallelogram -	2275
Short parallelogram -	1193

They were now hardened at a red heat, and tempered beyond the blue from the middle to within an inch of the extremities.

Experiment 37.

	Directive force.
Long parallelogram -	2277
Short parallelogram -	1865

If the mean of these two experiments be taken, it will be found, as was observed by COULOMB, that the directive force of a needle of a greater length than 5 inches is probably as its length.

My next object was to repeat the very interesting experiments recently published by Mr. BARLOW, proving the attraction of iron on a ship's compass to be dependant wholly on extent of surface. For this purpose I had three cylinders made of soft iron, about two inches and a half in diameter, and nearly the same in height. One of the cylinders was of sheet iron, less than the 20th of an inch in thickness; the second of that kind called chest plate, 0,185 inch thick; and the third was of solid wrought iron. The first weighed 2760, the second 9376, and the solid cylinder 22929 grains. Pre-

vious to the experiments they were all made red hot, to destroy any accidental magnetism.

The compass employed was of a very delicate construction, and the cylinder was so placed that its centre was in the direction of a tangent to the zero of the compass, and at the distance of 4,85 from the southern extremity of the needle. The position of the cylinder was varied six times, and the following were the deviations of the needle.

	Sheet iron cylinder.	Chest plate cylinder.	Solid cylinder.
	° ′	° ′	° ′
	2.15	2.50	2.55
	2.15	3. 4	3.15
	2.45	3.20	2.57
	2. 5	3.45	2.50
	2. 5	3.10	2.55
	2.10	3.30	2.30
Mean	2.16	3.16	2.54

Suspecting an error in the experiments with the solid cylinder from an accident which occurred, I repeated the whole with the utmost attention. The position of each cylinder was now varied eight times.

	Sheet iron cylinder.	Chest plate cylinder.	Solid cylinder.
	° ′	° ′	° ′
	2. 3	2.55	3.15
	2.22	2.50	3.12
	2.32	3.20	3.15
	2.20	3.40	3. 0
	1.50	3.40	3.15
	2.45	3.28	2.50
	2.45	3.10	2.45
	1.55	3. 5	2.58
Mean	2.19	3.16	3. 4

The surfaces of the cylinders determined by very careful measurement were, the sheet iron, 28,54; the chest plate, 30,77; and the solid cylinder, 28,94 inches.

Reducing the deviations to the same extent of surface, viz. that of the solid cylinder, they become respectively 141, 184, and 184 minutes.

These last results perfectly coincide with the deductions of Mr. BARLOW, that the effect of iron on a ship's compass is as the surface, and is wholly independent of the mass; but that a certain degree of thickness of the iron (about two tenths of an inch) is necessary to the complete developement of this effect.

The following are the principal inferences which may be drawn from the experiments I have detailed.

That the best material for compass needles is *clock spring;* but care must be taken in forming the needle to expose it as seldom as possible to heat, otherwise its capability of receiving magnetism will be much diminished.

That the best form for a compass needle is the *pierced rhombus,* in the proportion of about five inches in length to two inches in width, this form being susceptible of the greatest directive force.

That the best mode of tempering a compass needle is, first to harden it at a red heat, and then to soften it from the middle to about an inch from each extremity, by exposing it to a heat sufficient to cause the blue colour which arises again to disappear.

That in the same plate of steel of the size of a few square inches only, portions are found varying considerably in their

capability of receiving magnetism, though not apparently differing in any other respect.

That polishing the needle has no effect on its magnetism.

That the best mode of communicating magnetism to a needle, appears to be by placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets (the magnets being in the same line), and laying the magnets so joined, flat upon the needle with their poles upon its centre; then having elevated the distant extremities of the magnets, so that they may form an angle of about two or three degrees with the needle, they are to be drawn from the centre of the needle to the extremities, carefully preserving the same inclination, and having joined the poles of the magnets at a distance from the needle, the operation is to be repeated ten or twelve times on each surface.

That in needles from 5 to 8 inches in length, their weights being equal, the directive forces are nearly as the lengths.

That the directive force does not depend upon extent of surface, but in needles of nearly the same length and form, is as the mass.

That the deviation of a compass needle occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, on extent of surface, and is wholly independent of the mass, except a certain thickness of the iron, amounting to about two tenths of an inch, which is requisite for the complete developement of its attractive energy.

XI. *Notice respecting a volcanic appearance in the Moon, in a Letter addressed to the President. By Captain* Henry Kater *F. R. S.*

Read February 8, 1821.

Dear Sir,

London, February 8th, 1821.

It may perhaps be interesting to the Royal Society to be informed, that on Sunday evening, the 4th instant, I observed a luminous spot in the dark part of the moon, which I was inclined to ascribe to the eruption of a volcano.

The telescope used was an excellent Newtonian of $6\frac{1}{4}$ inches aperture, with a power of 74. The moon was exactly two days old, and the evening so clear, that I was able to discern the general outlines in the dark part of her disk. Her western azimuth was about 70°, and her altitude about 10 degrees.

In this position at 6 hours 30 minutes, the volcano was situated (estimating by the eye) as in the accompanying sketch. [See Plate X.] Its appearance was that of a small nebula subtending an angle of about 3 or 4 seconds.

Its brightness was very variable; a luminous point, like a small star of the 6th or 7th magnitude, would suddenly appear in its centre, and as suddenly disappear, and these changes would sometimes take place in the course of a few seconds.

On the evening of the 5th, having an engagement which prevented my observing it myself, I arranged the telescope

for two friends, who remarked the same phenomena as the night before, but in an inferior degree, partly perhaps in consequence of the evening not being so favourable.

On the 6th I again observed it; it had certainly become more faint, and the star-like appearance less frequent. I could see it very distinctly with a power of 40. As the moon approached the horizon, it was visible only at intervals when the star-like appearance took place. On the same evening I had the pleasure of showing it to Mr. HENRY BROWNE, F. R. S.

I regret that I had no micrometer adapted to my telescope; but I have reason to believe the distance of the volcano from the edge of the moon was about one tenth of her diameter, and the angle it formed this evening with a line joining the cusps was about 50°.

I remarked near the edge of the moon, a well known dark spot, from which the volcano was distant, as nearly as I could estimate, three times its distance from the edge of the moon.

In a map of the moon published by Dr. KITCHENER (and which is the best small map with which I am acquainted), there is a mountain sufficiently near the situation of the volcano, to authorize the supposition that they may be identical.

On the 7th I could still see the volcano, and the occasional star-like appearance; but I do not think it was sufficiently perceptible to have been discovered by a person ignorant of its precise situation. I am inclined however to think, that the difficulty of seeing it is rather to be attributed to the

increased light of the moon, than to the diminished action of the volcano.

I have the honour to be,

Dear Sir, &c. &c.

HENRY KATER

To Sir Humphry Davy, Bart. P. R. S. &c.

P. S. Since the preceding letter was written, I have ascertained that the spot in which I observed the volcanic appearance is that named ARISTARCHUS. This spot was particularly examined by HEVELIUS, who calls it Mons Porphyrites, and who considers it to be volcanic. If his drawings are to be relied upon, it has undergone a considerable change in its appearance since his time.

Sir WILLIAM HERSCHEL has recorded in the Philosophical Transactions an observation of three volcanoes, which he perceived in the moon, April 19th, 1787, at $10^h.36^m$, sidereal time. One of these, which he says showed " an actual eruption of fire or luminous matter," was distant from the northern limb of the moon $3'.57'',3$, the diameter of the burning part being not less than $3''$. I find that this observation was made about 9 o'clock in the evening, when the moon was not quite two days old; and from the situation of the spot described by Sir WILLIAM HERSCHEL, I have no doubt of its being the same that I have noticed.

J.s Basire sc.

XII. *A farther account of fossil bones discovered in caverns inclosed in the lime stone rocks at Plymouth.* By JOSEPH WHIDBEY, *Esq.* *In a Letter addressed to Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read February 8, 1821.

Bovisand Lodge, near Plymouth,
11th Nov. 1820.

DEAR SIR,

IN November 1816, I sent to Sir JOSEPH BANKS, some fossil bones found in the lime stone quarries at Oreston, near Plymouth, which bones were submitted by him to your examination; and as you considered them to be of some importance, a description of them was laid before the Royal Society, on the 27th of February, 1817.

I now take the liberty of sending you some more bones that have been subsequently found, nearly similarly situated, and not far from the place where the others were discovered, and I beg you will please to make use of these in any way you may think proper for the benefit of science.

These bones were lately found in a cavern one foot high, eighteen feet wide, and twenty feet long, lying on a thin bed of dry clay at the bottom; the cavern was entirely surrounded by compact lime stone rock, about eight feet above high water mark, fifty-five feet below the surface of the rock, one hundred and seventy-four yards from the original face of the quarries, and about one hundred and twenty

yards, in that direction, from the spot where the former bones were found in 1816.

All this quarry had been worked by blasting through the solid rock, with here and there a few small caverns similar to that where the bones were discovered, but none of them had the smallest appearance of ever having had any opening to the surface, or connection with it whatever, or with each other. The caverns here spoken of were quarried many feet below the bottom of them, and nothing was found but hard solid lime-stone, in which the quarrying ceased, and the workmen proceeded on in an horizontal direction.

Many caverns have been met with in these quarries, the insides of which have been crusted with stalactite; but there was no appearance of this kind in the cavern where the bones were found, every part of it being perfectly dry, and nearly clear of rubbish, a circumstance which clearly proves it had no connection with the surface, as in that case water would have found its way into it, the dropping of which would have formed stalactite, as in other instances.

I also send you some other bones, found about the same time, four yards distant from those just mentioned, and I have reason to believe on the same level, and under exactly similar circumstances.

To this account Sir Everard Home has added the following description of the bones, and the names of the animals to which they appear to have belonged.

1. The fourth grinder from the front, on the right side of the upper jaw, of the single horned rhinoceros.
2. The hindermost grinder but one, on the left side of the lower jaw, of the black or brown bear.
3. The hindermost grinder but one, on the right side of the upper jaw, of the black or brown bear.
4. Tusk of the left side of the lower jaw.

   Tusk of the left side of the upper jaw of the brown or black bear.
5. Portions of two tibiæ, apparently of the same animal.
6. One lumbar? vertebra.

   Portion of the os innominatum?

   Portion of the sacrum.

   Head of the os femoris.

   Two portions of cannon? bone of an animal of the deer kind.

   Portions of two dorsal vertebræ.

   Small portion of the pelvis, with part of the acetabulum.

   Part of the ulna.

   Part of the body of the os femoris.

All apparently of the same animal, which is of the size of a bear.

The bones are deposited in the Museum of the Royal College of Surgeons.

XIII. *On the aëriform compounds of Charcoal and Hydrogen; with an account of some additional experiments on the gases from oil and from coal.* By William Henry, *M. D. F. R. S. &c.*

Read February 22, 1821.

The experiments on the aëriform compounds of charcoal and hydrogen, described in the following pages, are supplementary to a Memoir on the same class of bodies, which the Royal Society did me the honour to insert in their Transactions for 1808, as well as to other papers on the same subject, which have been published in Mr. Nicholson's Journal, and in the Memoirs of the Manchester Society. Of these essays, I beg leave to offer a very brief recapitulation, with the view merely of connecting them with what is to follow.

In the first of these essays (Nicholson's Journal, 8vo. June, 1805), I detailed a series of experiments on the gases obtained by the destructive distillation of wood, peat, pit-coal, oil, wax, &c. from which it appeared that the fitness of those gases for artificial illumination was greater, as they required for combustion a greater proportional volume of oxygen; and that the gases generated from different inflammable bodies, or from the same inflammable substance under different circumstances, are not so many distinct species, which under such a view of the subject would be almost infinite in number, but are mixtures of a few well known gases, chiefly of carburetted hydrogen with variable pro-

portions of olefiant, simple hydrogen, sulphuretted hydrogen, carbonic acid, carbonic oxide, and azotic gases; and that the elastic fluids obtained from coal, oil, &c. have probably, in addition to these, an inflammable vapour diffused through them when recent, which is not removed by passing them through water.* In the same paper I explained certain anomalies that appear in the experiments of the late Mr. CRUICKSHANK, of Woolwich, which are not at all chargeable as errors upon that excellent chemist, and could only be elucidated by farther investigation of the gases to which they relate. Of his labours it would be unjust, indeed, to speak in any terms but those of approbation, for they may fairly be considered as the foundation of most that is now known respecting this species of aëriform bodies. To Mr. DALTON, also, we are indebted for an accurate acquaintance with carburetted hydrogen gas, and for much information that is valuable in assisting us to judge of the composition of mixed combustible gases, by the phenomena and results of firing them with oxygen.†

In the second Memoir (Philosophical Transactions, 1808), I described a series of experiments on the gases obtained from several different varieties of pit-coal, and from the same kind of coal under different circumstances. Various species of that mineral were found to yield aëriform products, differing greatly in specific gravity, combustibility, and illuminating power; the cannel coal of Wigan, in Lancashire, being best adapted to the purpose, and the stone-coal of South Wales the least so. In decomposing any one species of coal, the

* NICHOLSON'S Journal, 8vo. XI. 72.

† New System of Chemical Philosop y, *passim*.

gaseous fluids were ascertained not to be of uniform quality throughout the process, but to vary greatly at different stages; the heavier and more combustible gases coming over first, and the lighter and less combustible afterwards. By subsequent experiments on the gases obtained from coal on the large scale of manufacture, it was found that a similar decline in the value of the products takes place, but not to the same extent, owing, probably, to the greater uniformity of temperature, which is attainable in large operations.*

On the practical conclusions, which it was the object of the last mentioned Essay to establish, I forbear to dwell, because they are unconnected with my present purpose, which is limited to the chemical constitution of these compound gases, and to the methods of separating them accurately from each other. The view of their nature and composition, which was taken in the first Essay, was opposed by those able philosophers, M. Berthollet, and Dr. Murray, of Edinburgh, who both contended for greater latitude as to the proportions in which hydrogen and charcoal are capable of uniting, and considered these proportions indeed as subject to no limitation. The facts, however, which have since been multiplied in this, as well as in other departments of chemistry, tending to prove, that bodies capable of energetic combination unite in a few definite proportions only, leave little doubt that the same law holds good with respect to the compounds of hydrogen and charcoal. Not that it is meant that the known compounds of those elements are the only possible ones; for others will probably be discovered, which will still be found conformable to the general law, *that when one body combines with another in*

* Manchester Society's Memoirs, new Series, vol. III.

different proportions, the greater proportions are multiples of the less by an entire number.

A different view of the subject has lately been taken by the ingenious author of the Bakerian Lecture, published in the Philosophical Transactions for 1820. In that paper, Mr. Brande has endeavoured to prove, that the gas called light carburetted hydrogen, or simply carburetted hydrogen, or hydro-carburet, is not entitled to be considered as a distinct species; that the only aëriform compound of charcoal and hydrogen, which is with certainty known to exist, is the gas called olefiant, or bi-carburetted hydrogen; and that the gases evolved by heat from coal and oil, are in fact nothing more than mixtures of olefiant and simple hydrogen gases in various proportions.

In assuming, in the first Essay, the existence of light carburetted hydrogen as a definite compound, characterized by its requiring, for the complete combustion of each volume, two volumes of oxygen, and giving one volume of carbonic acid, I relied on the sole authority of Mr. Dalton; for the gas of marshes, though before known to be inflammable, had not been subjected to accurate examination by any other chemist. Mr. Cruickshank, indeed, speaks of it as "pure hydro-carbonate;"* but since he classes it in that respect with the gas obtained by the destructive distillation of camphor, from which it differs essentially in composition, it is plain that he was not correctly acquainted with the properties of pure carburetted hydrogen. Previously to the second set of experiments, I satisfied myself by the careful analysis of a specimen of the gas from stagnant water, for which I

* Nicholson's Journal, 4to. vol. V. p. 6.

was indebted to Mr. DALTON, that it really has the properties which have been ascribed to it by him as characteristic; and in 1807 I found precisely the same characters in the fire-damp of coal-mines.* Dr. THOMSON, also, from experiments in 1811,† on the gas from stagnant water, and Sir HUMPHRY DAVY,‡ from the analysis of the fire-damp in 1815, drew the same conclusions. It is in the power, indeed, of every chemist to investigate for himself the properties and composition of carburetted hydrogen gas, since it may easily be procured in considerable quantity, by stirring the bottom of almost any stagnant pool, especially if composed of clay. During the last summer, I obtained it from a source of this kind, which afforded it in such abundance, that several gallons might have been collected in a few minutes. This gas I submitted to repeated and most careful examination. It contained $\frac{1}{20}$th its volume of carbonic acid, but no sulphuretted hydrogen whatever, and no proportion of oxygen gas that could be discovered by attentively testing it with nitrous gas. The results of its combustion with oxygen gas, effected in a Volta's eudiometer in the usual manner, showed that it was contaminated with $\frac{1}{15}$th its volume of azotic gas. Apart, however, from this, the pure portion, in a great number of trials, required, as nearly as can be expected in experiments of this sort, two volumes of oxygen for combustion, and gave one volume of carbonic acid. Its specific gravity, taken on quantities procured at three several times, varied only from ·582 to ·586, the mean of which is ·584; and this, allowing for $\frac{1}{15}$th of azotic gas of specific gravity ·972,

* NICHOLSON's Journal, 8vo. XIX. 149.

† Mem. of the Wernerian Society, I. 506.

‡ Phil. Trans. 1816, p. 5.

gives ·556 for the specific gravity of pure carburetted hydrogen gas, a number which coincides almost exactly with that found by Dr. THOMSON.* Since, therefore, the same results have been obtained from the examination of gases similarly collected at distant times and places, there appears to me no reason for refusing to consider carburetted hydrogen gas as a true chemical compound, characterized by perfect uniformity of properties and composition. At the temperature of 60° Fahrenheit, and under 30 inches pressure, 100 cubical inches must weigh 16·95 grains, and be composed (taking the weight of 100 cubic inches of carbonic acid at 46·5 grains, and the charcoal in 100 grains of that acid at 27·3 grains) of

	Grains.	Grains.	Grains.
Charcoal	12·69	74·87	100
Hydrogen	4·26	25·13	33·41
	16·95	100.	133·41

And olefiant gas (giving twice its volume of oxygen by combustion, and weighing 29·64 grains for 100 cubical inches†) must be constituted of

	Grains.	Grains.	Grains.
Charcoal	25·38	85.63	100
Hydrogen	4·26	14.37	16·71
	29·64	100.	116·71

And as 16·7 is to 100, so very nearly is 1 to 6, which

* Annals of Philosophy, Vol. XVI. p. 252.

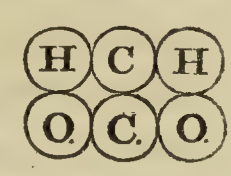
† I adopt this result of Dr. THOMSON from its near coincidence with that of an experiment of my own, on the specific gravity of olefiant gas, published in the Phil. Trans. 1808, p. 293.

last number is the weight of the atom of charcoal, as deduced from the constitution of olefiant gas. It is true, that this determination a little exceeds that which is derived from the composition of carbonic acid (viz. 5·65), the atom of oxygen being taken at 7·5. But if 8 be the true number for oxygen, which now seems to be most probable both from experiment and analogy, we shall then find an exact coincidence between the relative weight of the atom of charcoal, as deduced from olefiant gas, and as determined from carbonic acid. Perhaps the true specific gravity of hydrogen gas, on which depend the relative weights of the atoms of hydrogen and oxygen, may be fully as correctly ascertained from the composition of carburetted hydrogen, as by direct attempts to weigh so light a fluid. Now, as the hydrogen in 100 cubic inches of hydro-carburet weighs only 4·26 grains, and is equivalent to 200 cubic inches of hydrogen gas, we have 2·13 grains for the weight of 100 cubic inches of hydrogen gas, from which may be deduced ·0698 for its specific gravity, that of air being 1. And if the specific gravity of oxygen gas be 1·111, it will be found that the two volumes of hydrogen, required to saturate one volume of oxygen gas, have as nearly as possible the relative weight of 1 to 8.

Were any additional argument necessary to establish the existence of carburetted hydrogen as a distinct species, it might be derived from the action of water on that gas, which, besides being absorbable in a constant proportion, admits of being expelled again by the application of heat, not otherwise changed than in having acquired a small quantity of those gases which are always present in water, and of

which it is impossible to deprive it even by long continued boiling.

The process, by which carburetted hydrogen gas is evolved in natural operations, is no doubt the decomposition of water, and admits of being explained on the atomic theory of Mr. Dalton, by supposing two atoms of charcoal to act at once on two atoms of water. One atom of charcoal attracts the two atoms of hydrogen, forming carburetted hydrogen gas, and the other atom of charcoal unites with two atoms of oxygen, constituting carbonic acid. This is illustrated by the annexed figure, in which two atoms of charcoal C.C. are represented as interposed between two atoms of water, each consisting of an atom of hydrogen and an atom of oxygen.



Dividing the diagram vertically into three parts, we have the original substances; and separating it horizontally, we obtain the two new compounds. This theoretical view of the subject is confirmed by the fact, that the carburetted hydrogen, formed at the bottom of stagnant pools, is never accompanied by carbonic oxide, but always by carbonic acid, the full quantity of which is prevented from appearing, in consequence of the absorption of a great part of it by the mass of water, under which the changes are taking place.

Being provided with such an abundant supply of carburetted hydrogen, I availed myself of it to examine the mutual action of that gas and chlorine on each other, principally with a view to ascertain, how far reliance may be placed on the latter as an instrument in the analysis of mixed combustible gases. This is a part of the subject that was first investigated, though with a different view, by Mr. Cruick-

shank.* He observed that a mixture of chlorine with hydrogen, carburetted hydrogen, or carbonic oxide in certain proportions, kept in a bottle entirely filled with the mixture, and furnished with an air-tight stopper, did not exhibit any immediate action, but that in twenty-four hours, on withdrawing the stopper, the fluid immediately rushed in, and filled most of the space originally occupied by the gases. But he was not aware of the influence of light on these changes, which was discovered about the same time by Gay Lussac† and by Dalton.‡ It does not, however, appear to have been ascertained by either of them, whether the complete exclusion of light prevents any degree of action of chlorine and carburetted hydrogen on each other. I mixed, therefore, those two gases in different proportions in well stopped vials, which were completely filled with the mixture, and covered by opake cases. When the stoppers were removed under water, at various intervals after the mixture, from a few minutes to 39 days, no diminution whatever of volume was found to have taken place; and after having removed the chlorine by liquid potash, the carburetted hydrogen gas gave the usual products of carbonic acid, and consumed the usual proportion of oxygen. Mixtures also of hydrogen and chlorine, and of carburetted hydrogen and chlorine, standing over water in graduated tubes, which were shaded by opake covers, sustained no loss of bulk, except what arose from the absorption of chlorine by the water, the combustible gas remaining wholly unaltered. It may be

* Nicholson's Journal, 4to. V. 202.

† Mem. de la Soc. d'Arcueil. II. 349.

‡ New System of Chemical Philosophy, p. 300.

considered, therefore, as quite essential to the mutual agency of these gases, that they should be subjected to the influence of light. But it is not necessary that the direct rays of the sun should fall on the mixture, the light of a dull and cloudy day being fully adequate to the effect. On a day of this sort, I filled several stoppered vials, graduated into hundredths of a cubic inch, with a mixture of 30 volumes of carburetted hydrogen with from 80 to 90 of chlorine, and uncovering them all at the same moment, exposed them to the feeble light which was then abroad. By exposure of one of the vials during half a minute, no diminution of volume was found to have been effected; another vial, opened under water when one minute had elapsed, showed an absorption of five parts; a third in two minutes had lost 15 parts; a fourth in four minutes 25 parts; and a fifth, opened in five minutes, contained only 50 volumes out of the original 110.

The products, resulting from the contact of carburetted hydrogen and chlorine, under circumstances favourable to their mutual action, have been described by Mr. Cruickshank, with whose experience on this point my own entirely agrees When rather more than four volumes of chlorine are kept in mixture with one volume of gas from stagnant water, the products are muriatic acid gas, and a volume of carbonic acid equivalent to that of the pure carburetted hydrogen; and this, whether the mixture be exposed to direct or indirect solar light; the only difference being that the less intense the light, the more slowly is the effect produced. When less than four volumes of chlorine are employed, the residue consists of muriatic and carbonic acids, carbonic oxide,

and undecomposed carburetted hydrogen, the proportions of the two last increasing as, within certain limits, we reduce the relative quantity of chlorine. These changes were ascertained, both by Dr. DAVY and the late Dr. MURRAY,* to depend on the presence of moisture, which is unavoidably introduced in the common mode of operating; for when the gases, first perfectly dried, were mixed in an exhausted glass vessel, and exposed even to the direct rays of the sun, no mutual action was found to ensue. In the theory of these changes there is, it must be confessed, a little uncertainty. Does the chlorine, it may be asked, act simultaneously on the hydrogen of water, and on that of the combustible gas; or does it decompose water only? The former view of the subject appears to me most probable, because, if the chlorine acted on water only, free hydrogen would be evolved from that portion of the hydrocarburet which abandons its charcoal to the oxygen of the water; which is not consistent with experience. When it is required to form carbonic acid, four volumes of chlorine must be used for the decomposition of each volume of carburetted hydrogen. In this case, two atoms of chlorine unite with the two atoms of hydrogen existing in the combustible gas, and the two other atoms of chlorine with the two atoms of hydrogen from the water. But to convert carburetted hydrogen into carbonic oxide, three atoms of chlorine are sufficient, two of which are employed, as in the first case, and the third is expended in saturating the hydrogen of one atom of water, which supplies to the charcoal an atom of oxygen for the formation of carbonic oxide. Calculating in the same

* NICHOLSON's Journal, xxviii. 143, and 201.

manner, we shall find, also, that three atoms of chlorine are adequate to convert one atom of carbonic oxide into carbonic acid.

The facts which have been stated sufficiently prove, that chlorine cannot be employed as a means of correctly analyzing mixtures of olefiant gas, either with hydrogen or with carburetted hydrogen, if light be admitted, even though of feeble intensity, and for the short interval during which such an experiment may be expected to continue: and they explain that uncertainty as to the results of analyses of mixed gases made in this way, which was first remarked by Mr. Faraday* and subsequently by myself.† Chlorine becomes, however, a most useful agent in separating olefiant gas from such mixtures, provided light be entirely excluded during its operation, as I have found by subjecting to its action, mixtures of those gases with known proportions of olefiant gas. In these analytical experiments, I admitted into a graduated tube standing over water, a volume of chlorine exceeding by about one half what was known to be sufficient, and noted its bulk when actually in the tube, which was immediately shaded by an opake cover. A measured quantity of the mixture was then passed up, and in about ten minutes the outer cover was cautiously lifted, till the surface of the water appeared. The diminution of volume thus ascertained, divided by 2, was found to give pretty correctly the quantity of olefiant gas known to be contained in the mixture. But the greatest precision was attained by waiting 15 or 20 minutes, and then quickly washing the remaining gas with dilute solution of potash, in order to remove the excess of chlorine.

* Journal of Science, &c. vi. 358. † Manchester Memoirs, new Series, vol. iii.

From the volume of the residuary gas, it was necessary to deduct the amount of impurity previously ascertained to exist in the chlorine ; and the remainder, taken from the volume of mixed gases which had been operated on, showed how much olefiant gas had been condensed by the chlorine. When very narrow tubes were employed, and the column of gases mixed with chlorine was of considerable length, a longer continuance of the experiment was found necessary, and the gases were suffered to remain in contact during an hour or more. In this way it was ascertained, that olefiant gas may be accurately separated by chlorine from hydrogen, carburetted hydrogen, or carbonic oxide gases, or from mixtures of two or more of those gases, which are left quite unchanged in volume and in chemical properties, when light has been carefully excluded from the mixture.

This property of chlorine is the foundation of a fresh analysis, to which I have thought it expedient to submit the gases from coal and oil, in order to decide what aëriform fluids remain after the separation of that portion which is condensible by chlorine ;—whether the residue consists, as I have heretofore maintained, of carburetted hydrogen chiefly, with variable proportions of hydrogen and carbonic oxide ; or whether, according to the new view of the subject, it consists of hydrogen gas only.

In the experiments made for this purpose, I operated generally on from 60 to 80 cubic inches of oil gas or coal gas, assaying a small specimen first, as a guide to the quantity of chlorine which it would be necessary to employ. The volume of chlorine thus found to be requisite, and about half as much more, was passed into an air receiver standing over water, and

completely shaded by an opake cover which was fitted over it. The oil or coal gas was then added by degrees, if much condensation was expected, because in that case a considerable increase of temperature would have been produced by the sudden admixture of large quantities; or at once, if only a moderate action had been indicated by the previous assay. The mixture was allowed to stand, completely guarded from the light, during 30 or 40 minutes, or even longer, and the residue was expeditiously washed with liquid potash, and a small portion again assayed, to ascertain that the action of the chlorine was complete. The specific gravity of the washed gas was then carefully taken, that of the entire gas having been previously determined: and the results of its combustion with oxygen examined, and compared with those of the gas in its original state.

Experiments on the gas from oil.

In obtaining this gas at different times, I used the same kind of whale oil, which had been heated a little below its boiling point during two hours, in order to deprive it of water. The oil was admitted by drops into an ignited iron tube filled with fragments of broken crucibles, and no difference, that I am aware of, existed in the circumstances under which the decomposition was effected, except that the degree of heat was purposely lowered in the latter processes, till that temperature was attained, which was barely adequate to the production of gas. The oil gas procured from London, I obtained through the kindness of Mr. Richard Phillips. It had been prepared from cod oil, at the manufactory of Messrs. John and Philip Taylor, and having been conveyed to Manchester in bottles accurately stoppered and tied over with a double

fold of bladder, it was found not to have acquired any admixture with atmospheric air. The results are contained in the following table, in which the expression *entire gas* is applied to the gas precisely as it came over, except that the carbonic acid had been removed by liquid potash, applied in the smallest quantity and with the least agitation that were adequate to the effect.

TABLE I. *Containing the results of experiments on the gas obtained from whale oil.*

Entire Gas.					Residue left by chlorine.		
			100 vols.			100 vols.	
No. of Experiment.	Sp. Gr.	100 vols. lose by chlorine.	take oxyg.	give carb. ac.	Sp. Gr.	Take Oxyg.	Give Carb. ac.
1	.464	6	116	61	.4107	94	46
2	.590	19	178	100	.4400	108	58
3	.758	22.5	220	130	.6160	145	85
4(London)	.906	38	260	158	.6060	152	91

From the foregoing table it appears, that the gas obtained at different times from oil of the same quality, is far from being of uniform composition, and that great differences, as to its specific gravity and chemical properties, are occasioned by the temperature at which it is produced. So far as my experience goes, no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases; but the lower the heat that is employed, provided it be adequate to the effect, the heavier and more combustible is the gas, and the better suited to artificial illumination.

From the experiments which I published in 1805, and which were made on a single specimen of oil gas, I was led to consider it as constituted of one volume of olefiant gas

with seven volumes of mixed gases, of which the greatest part was carburetted hydrogen. Mr. DALTON has since favoured me with a specimen of oil gas prepared by himself, which contained in 100 parts, 40 of a gas condensible by chlorine; and it appears from the table that oil gas, manufactured on the large scale, may contain in 100 parts, 38 parts of a gas similarly characterized.* It is not improbable indeed that by a temperature carefully regulated, the whole of the aëriform fluids may be obtained from oil, of such quality as to be entirely condensible by chlorine; and from the great superiority of the light which such a gas would afford, and the reduction that might be effected in the capacity of the gasometers, the discovery of a mode of producing it in this state, would be an important practical improvement.

The inferences respecting the nature of the gas from oil, I reserve till after the account of the experiments on coal gas, as the same remarks, with some slight modifications, will apply to both cases.

Experiments on the gas from coal.

The numerous experiments and observations on the gas from coal, which I have already published, appear to me to preclude the necessity of going much into the subject on this occasion. What I have lately had in view, has been to render the analysis of this gas more complete, by a careful exami-

* Since this Paper was written, I have received from Mr. PHILLIPS a second specimen of oil gas prepared by Messrs. TAYLOR. It contains in every 100 volumes, 42 or 43 parts of gas condensible by chlorine; but in other respects very nearly agrees, (making allowance for the greater proportion of that ingredient) with the gas described in the text.

nation of that portion of it which remains after the action of chlorine. The gas, submitted to these recent experiments, was prepared from Wigan cannel, at the manufactory of Messrs. PHILIPS and LEE. It was collected from an opening in a pipe between the retort and the tar-pit, generally about an hour after the commencement of the distillation, except in the instance of the gas No. 4, which was taken five hours, and No. 5, which was taken ten hours, from that period. Before using it, the carbonic acid and sulphuretted hydrogen, which were always present in the early products, were separated by careful ablution with liquid potash. As the gas No. 5, was not at all diminished by chlorine, it was obviously unnecessary to examine it in any but its entire state.

TABLE II. *Containing the results of experiments on the gas obtained from coal.*

Entire Gas.					Gas left by Chlorine.		
Experiment.	Sp. Gr.	100 volumes. Take oxyg.	100 volumes. Give car. ac.	Loss by Chlorine.	Sp. Gr.	100 volumes. Take oxyg.	100 volumes. Give car. acid.
1	.650	217	128	13	·575	178	92
2	.620	194	106	12	·527	160	82
3	.630	196	108	12	·535	148	80½
4	·500	166	93	7	·450	140	75
5	·345	78	30	0			

Inferences respecting the composition of that part of the gases from coal and from oil, which is not condensible by the action of chlorine.

The analytical experiments, which I have described on the action of chlorine on artificial mixtures of olefiant with hydrogen and carburetted hydrogen gases, afford no room for

doubt that by that agent the quantity of olefiant gas in any mixture of these gases may be accurately determined. We are not, however, acquainted with any chemical agent, either liquid or aëriform, which, from a mixture of hydrogen, carburetted hydrogen, and carbonic oxide, is capable of separating one of those gases, leaving the others in their original state and quantity.* The only method at present known of determining the composition of such a mixture is by firing it with oxygen gas, and, from the phenomena and results of the process, deducing the proportion of its ingredients. In drawing conclusions of this kind, it is necessary to have distinctly in view the properties of those gases in their separate state. The following Table contains an abstract of their leading characters, which will be found very useful in such investigations. Though not strictly necessary, I have included olefiant gas, in order to render the Table more complete.

TABLE III. *Exhibiting the characteristic properties of different combustible gases.*

Names of Gases.	Sp. Gr. Air 1000	100 Volumes require Oxygen.	Total.	Diminished by Firing.	Carb. Acid produced.
Olefiant Gas	·970	300	400	$200 = \frac{1}{2}$	200
Carburetted Hydrogen	·556	200	300	$200 = \frac{2}{3}$	100
Hydrogen Gas	·069	50	150	$150 = \frac{3}{3}$	0
Carbonic Oxide	·972	50	150	$50 = \frac{1}{3}$	100

* I have not found that chlorine can be employed with any success in analyzing such mixtures; for when placed in contact with two or more of those gases, and exposed to light, it does not act upon one exclusively, but upon all that compose the mixture.

As an illustration of the method of investigating the proportions of mixtures of the three last gases, we may take the instance of a mixed gas, free from olefiant gas, of specific gravity ·534, of which 100 volumes consume 110 of oxygen, and afford 70 of carbonic acid, the diminution of the whole 210 after firing being 140 volumes. Now it must be obvious from inspection of the Table, that the 70 parts of carbonic acid cannot all have resulted from the combustion of carburetted hydrogen, since, for the saturation of 70 measures of that gas, 140 of oxygen would have been required, whereas only 110 have been expended. We may therefore safely infer the presence of carbonic oxide, a gas which, by combustion, gives its own volume of carbonic acid, with the expenditure of only half its volume of oxygen. The specific gravity of the specimen being lower than that of carburetted hydrogen, indicates also an admixture of simple hydrogen gas; and of this the proportion must necessarily be considerable, to countervail the weight of the heavy carbonic oxide. The following proportions of the three gases will be found to coincide with the properties of the mixture.

	Consume Ox.	Give Carb. Ac.	Dimin. by Firing.
40 vols. of carb. hydrogen	80	40	80
30 ——— carb. oxide	15	30	15
30 ——— hydrogen gas	15	0	45
100	110	70	140

No reliance, however, can be placed on the accuracy of such estimates, unless the specific gravity of the specimen agrees with that of the hypothetical mixture, as deduced from the proportion of its ingredients. But when this coincidence takes place, we have all the evidence, which the

subject at present admits, of the nature of the mixture; and as this agreement between experiment and calculation was found to take place very nearly, in all the instances comprehended in the two following Tables, we may consider the numbers composing them, as expressing, with sufficient exactness, the relative proportion of different gases in the residues of oil and coal gas left by the action of chlorine.

TABLE IV. *Showing the composition of* 100 *volumes of the gas remaining after the action of chlorine on oil gas.*

Exp.	Azote.	Carb. Hydr.	Carb. Oxide	Hydr. Gas.	Total.
1	7	30	15	48	100
2	5	40	15	40	100
3	5	65	20	10	100
4	5	75	15	5	100

TABLE V. *Showing the composition of* 100 *volumes of the gas remaining after the action of chlorine on coal gas.*

Exp.	Azote.	Carb. Hydr.	Carb. Oxide	Hydr. Gas.	Total.
1	1.5	94.5	4	0	100
2	6	82	2	10	100
3	2	66	14	18	100
4	5	60	12	23	100
5	10	20	10	60	100

It appears from the two foregoing Tables, that the portion of oil gas and coal gas, which is not condensible by chlorine, is in every case a mixed gas, consisting in most instances of carburetted hydrogen, carbonic oxide, and hydrogen, with a little azote, part of which may be traced to the impurity of the chlorine. In the best specimens of oil gas, the carbonic

oxide is in greater proportion than in the best kinds of gas from coal, and the carburetted hydrogen is most abundant in the latter gas. This, however, is more than compensated, so far as their illuminating power is concerned, by the greater richness of the aëriform products of oil in that denser species of gas, which is separable by chlorine. The proportion of hydrogen, both in oil gas and coal gas, appears to increase as they are formed at a higher temperature, and is always greatest in the latter portions of the gas from coal. But no instance has ever occurred to me of a gas obtained from oil or from coal, which, after the action of chlorine upon it with the exclusion of light, presented a residuum at all approaching to simple hydrogen gas; nor do I believe that such a gas can be generated under any circumstances of temperature, by which the decomposition of coal or of oil is capable of being effected.

Inferences respecting the composition of that part of the gas from coal and oil, which is condensed by contact with chlorine.

When a given volume of a mixture of olefiant and carburetted hydrogen gases is fired with oxygen, and an equal volume of the same mixture is first deprived of olefiant gas by the action of chlorine, and then fired with oxygen, it must necessarily happen that the excess of oxygen spent in the first combustion, above that consumed in the second, will be three times the volume of the olefiant gas, and that the excess of carbonic acid formed in the first experiment above that generated in the second, will be double the volume of the olefiant gas. A remarkable anomaly however, was, during the last summer, observed by Mr. DALTON in the

results of the combustion of a quantity of gas, which he had himself prepared from oil. One volume was found to consume three volumes of oxygen, and to yield little short of two volumes of carbonic acid, in those respects agreeing nearly with olefiant gas; but when mingled with more than the requisite proportion of chlorine, it was not, as olefiant gas would have been, entirely condensed, but suffered a diminution of only four tenths of its bulk, the remaining six tenths, after being freed from the redundant chlorine, agreeing in its properties with carburetted hydrogen. For example, 10 volumes of this gas (containing four of gas condensible by chlorine and six of carburetted hydrogen) consumed 30 volumes of oxygen, and gave 18 of carbonic acid. But of the oxygen, 12 volumes are due to the six of carburetted hydrogen, leaving 18 volumes for the combustion of the four volumes of gas condensible by chlorine, which is in the proportion of $4\frac{1}{2}$ to 1. Of the 18 volumes of carbonic acid, also, six may be traced to the combustion of the carburetted hydrogen, leaving 12 volumes as the product of four of the condensible gas, or in the proportion of 3 to 1. The portion of gas, condensed by the action of chlorine presents, therefore, decided differences from olefiant gas, in requiring not three only, but $4\frac{1}{2}$ volumes of oxygen for combustion, and in affording 3, instead of 2 volumes of carbonic acid. Nearly the same relation of the oxygen consumed, and carbonic acid produced, to that part of the gases from coal and oil which is condensible by chlorine, existed also not only in other experiments of Mr. Dalton, but in all those which I have myself made. The proportions I have found to vary in different cases from $4\frac{1}{2}$ to 5 volumes of

oxygen, and from $2\frac{1}{2}$ to 3 volumes of carbonic acid for each volume of the condensible gas.

On comparing also the specific gravity of the gases from coal and oil, as ascertained by experiment, with that which ought to result from mixtures of the residue left by chlorine, with such a proportion of olefiant gas as is deducible from analysis, I have invariably found, that the real specific gravity has considerably exceeded the estimated. For instance, the London oil gas was composed of 38 volumes of a gas condensible by chlorine, and 62 volumes of mixed gases not characterized by that property, and having the specific gravity .606. But 62 volumes of gas of specific gravity .606, mixed with 38 volumes of olefiant gas of specific gravity .970, should give a mixture of the specific gravity .754, instead of .906, which was the actual specific gravity of the entire oil gas. It will be found on calculation that the 38 volumes of gas, in order to make up the real specific gravity of the oil gas, must have had the specific gravity of 1.4 very nearly. This is the highest number that is deducible from my experiments for the specific gravity of that portion of oil gas or coal gas, which is condensed by the action of chlorine. In other instances, it varied from that number down to 1.2, but in every case its weight surpassed that of common air.

It is evident from these facts that the aëriform ingredient of oil gas and coal gas, which is reducible to a liquid form by chlorine, is not identical with the olefiant gas obtained by the action of sulphuric acid on alcohol, but considerably exceeds that gas in specific gravity and combustibility. Two views may be taken of its nature; for it may either be a gas *sui generis*, hitherto unknown, and constituted of hydrogen and

charcoal in different proportions from those composing any known compound of those elements;—or it may be merely the vapour of a highly volatile oil, mingled in various proportions with olefiant gas, carburetted hydrogen, and the other combustible gases. Of these two opinions, Mr. DALTON is inclined to the first, considering it as supported by the fact that oil gas, or coal gas, may be passed through water, without being deprived of the ingredient in question; and that this anomalous elastic fluid is absorbed by agitation with water, and again expelled by heat or other gases, unchanged as to its chemical properties, as we have both satisfied ourselves by repeated experiments. On the other hand, I have found that hydrogen gas, by remaining several days in narrow tubes in contact with fluid naphtha, acquires the property of being affected by chlorine precisely as if it were mixed with a small proportion of olefiant gas; and I am informed by Dr. HOPE, that oil gas, when forcibly compressed in GORDON's portable gas lamp, deposits a portion of a highly volatile essential oil. The smell also of the liquid which is condensed on the inner surface of a glass receiver, in which oil gas or coal gas has been mixed with chlorine, denotes the presence of chloric ether, evidently however mingled with the odour of some other fluid, which seems to me to bear most resemblance to that of spirit of turpentine. This part of the subject is well worthy of farther investigation; but having devoted to the enquiry all the leisure which I am now able to command, I must remain satisfied at present with such conclusions as are safely deducible from the foregoing investigation. These may be briefly recapitulated as follows:

1. That carburetted hydrogen gas must still be considered

as a distinct species, requiring for the perfect combustion of each volume two volumes of oxygen, and affording one volume of carbonic acid; and that if olefiant gas be considered as constituted of one atom of charcoal united with one atom of hydrogen, carburetted hydrogen must consist of one atom of charcoal in combination with two atoms of hydrogen.

2. That there is a marked distinction between the action of chlorine on olefiant gas, (which, in certain proportions, is entirely independent of the presence of light, and is attended with the speedy condensation of the two gases into chloric ether), and its relation to hydrogen, carburetted hydrogen, and carbonic oxide gases, on all which it is inefficient, provided light be perfectly excluded from the mixture.

3. That since chlorine, under these circumstances, condenses olefiant gas without acting on the other three gases, it may be employed in the correct separation of the former from one or more of the three latter.

4. That the gases evolved by heat from coal and from oil, though extremely uncertain as to the proportions of their ingredients, consist essentially of carburetted hydrogen, with variable proportions of hydrogen and carbonic oxide; and that they owe, moreover, much of their illuminating power to an elastic fluid, which resembles olefiant gas in the property of being speedily condensed by chlorine.

5. That the portion of oil gas and coal gas, which chlorine thus converts into a liquid form, does not precisely agree with olefiant gas in its other properties; but requires, for the combustion of each volume, nearly two volumes of oxygen more than are sufficient for saturating one volume of olefiant gas, and affords one additional volume of carbonic acid. It is

probably, therefore, either a mixture of olefiant gas with a heavier and more combustible gas or vapour, or a new gas *sui generis,* consisting of hydrogen and charcoal, in proportions that remain to be determined.

Manchester, January 1821.

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BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCXXI. a

METEOROLOGICAL JOURNAL
for January, 1820.

1820	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Jan. 1	8 0	26	43	29,42	23	Rain this Month 0,444 Inches.	NW	1	Cloudy and foggy.
	2 0	28	42	29,49	29		NW	1	Cloudy.
2	8 0	31	40	29,60	24		S	1	Fine.
	2 0	37	39	29,52	38		SW	1	Cloudy.
3	8 0	34	40	29,52	33		SW	1	Cloudy, some snow.
	2 0	33	48	29,86	37		NW	1	Fine.
4	8 0	27	41	30,07	27		SW	1	Fog.
	2 0	32	57	30,05	31			1	Fine, rather hazy.
5	8 0	19	35	30,14	20		W	1	Foggy.
	2 0	30	44	30,16	34		W	1	Cloudy.
6	8 0	28	37	30,11	26		W	1	Cloudy.
	2 0	35	48	30,12	37		SSW	1	Cloudy and hazy.
7	8 0	34	41	30,22	33		SE	1	Cloudy.
	2 0	30	47	30,39	35		E	1	Cloudy.
8	8 0	27	42	30,52	27		N	1	Fine, rather hazy.
	2 0	29½	45	30,55	32		N	1	Cloudy.
9	8 0	24	39	30,68	25		N	1	Cloudy, snow in the night
	2 0	33	39	30,63	34		N	1	Cloudy.
10	8 0	23	37	30,51	23		S	1	Cloudy, snow in the night
	2 0	27	45	30,45	29		S	1	Cloudy, some snow.
11	8 0	26	38	29.98	23		S	1	Cloudy, snow in the night
	2 0	32	47	29,73	31		S	1	Snow.
12	8 0	37	39	29,97	35		E	1	Cloudy.
	2 0	26	46	30,13	29		E	1	Cloudy.
13	8 0	21	31	30,14	20			1	Thick fog.
	2 0	28	42	30,13	39		E	1	Cloudy and hazy.
14	8 0	26	39	30,19	33		E	1	Cloudy.
	2 0	27	40	30,16	38		E	1	Cloudy.
15	8 0	25	34	29,87	23½			1	Cloudy.
	2 0	23	39	29,73	37		E	1	Cloudy.
16	8 0	25	37	29,68	23				Cloudy and foggy.
	2 0	28	31	29,71	34		NW		Cloudy.

METEOROLOGICAL JOURNAL

for January, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	o	o	Inches.			Points.	Str.	
Jan. 17	8	0	31	35	29,65	26	Rain this Month 0,444 Inches.	W	1	Cloudy and foggy.
	2	0	33	44	29,56	35		W	1	Cloudy.
18	8	0	29	36	29,37	28½			1	Cloudy, snow in the night
	2	0	32	45	29,34	33		E	1	Cloudy.
19	8	0	47	43	28,84	33		E	1	Cloudy, rain in the night.
	2	0	37	47	28,94	47		W	1	Cloudy.
20	8	0	36	43	29,52	32		W	1	Cloudy.
	2	0	33	57	29,41	35		W	1	Cloudy.
21	8	0	33	45	29,15	31½		W	1	Cloudy.
	2	0	36	50	29,31	37		SSE	1	Cloudy.
22	8	0	29	43	30,03	27		SE	1	Cloudy.
	2	0	32	48	30,12	34½		ESE	1	Cloudy and hazy.
23	8	0	32	41	30,11	27		ESE	1	Cloudy.
	2	0	39	43	30,07	39		SbyW	1	Fine.
24	8	0	38	42	29,84	35		S	2,3	Cloudy.
	2	0	43	45	29,74	43		SW	1,2	Rain.
25	8	0	43	45	29,86	38		NE	1	Rain.
	2	0	42	53	29,66	46		NNE	1	Rain.
26	8	0	44	48	29,48	40		W	1	Cloudy.
	2	0	48	53	29,62	46		W	1	Cloudy.
27	8	0	48	50	29,58	45		W	1,2	Fine.
	2	0	48	57	29,51	53		W	2	Cloudy.
28	8	0	43	52	29,55	43		N	1	Cloudy.
	2	0	45	54	29,78	47		SE	1	Fine.
29	8	0	40	52	30,17	40		N	1	Cloudy.
	2	0	33	54	30,17	36		N	1	Fine.
30	8	0	41	51	30,07	39		N	1	Cloudy.
	2	0	44	56	30,05	46		N	1	Cloudy.
31	8	0	42	50	30,00	41		S	1,2	Fine.
	2	0	46	56	30,00	51		SW	1	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Feb. 1	8	0	38	51	29,99	37	Rain this Month 1,425 Inches.	SbyE	1	Fine, rather hazy.
	2	0	45	59	29,90	51		E	1	Fine.
2	8	0	34	50	29,83	34		E	1	Foggy.
	2	0	40	56	29,86	48		E	1	Hazy and cloudy.
3	8	0	35	50	29,96	34		N	1	Hazy.
	2	0	36	54	29,99	37		N	1	Hazy.
4	8	0	34	48	30,04	34		W	1	Hazy and cloudy.
	2	0	38	51	30,04	38		SbyE	1	Cloudy.
5	8	0	39	47	29,96	35		S	2	Cloudy.
	2	0	44	52	29,91	43		S	1	Rain.
6	8	0	43	49	29,82	42		S	1	Cloudy.
	2	0	46	49	29,91	47		S	1	Cloudy.
7	8	0	47	49	30,11	47		SW	1	Cloudy.
	2	0	48	52	30,12	49		S	1	Cloudy.
8	8	0	44	52	30,14	43		W	1	Cloudy.
	2	0	45	55	30,15	45		SW	1	Cloudy.
9	8	0	42	50	30,05	41		S	1.2	Fine.
	2	0	50	57	29,92	52		S	1	Fine.
10	8	0	45	54	29,85	41		W	1	Cloudy.
	2	0	47	52	29,96	50		SW	1	Fine.
11	8	0	37	51	30,12	35		W	1	Cloudy.
	2	0	48	55	30,02			S	1	Cloudy.
12	8	0	41	52	29,85	40		S	1	Cloudy and hazy.
	2	0	43	54	29,94	44		SW	1	Cloudy.
13	8	0	40	51	30,03	39		S	1	Cloudy.
	2	0	44	50	30,00	42		S	1	Fine.
14	8	0	37	49	30,18	36		NE	1	Cloudy.
	2	0	48	54	30,23	48		N	1	Cloudy.
15	8	0	36	47	30,32	34		S	1	Cloudy.
	2	0	41	54	30,31	43		NbyE	1	Fine.

METEOROLOGICAL JOURNAL

for February, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Feb. 16	8	0	28	40	30,27	30	Rain this Month 1,425 Inches.	N	1	Cloudy.
	2	0	40	42	30,24	38		N	1	Fine.
17	8	0	29	43	30,11	29		NW	1	Foggy.
	2	0	41	51	30,08	43		NW	1	Fine.
18	8	0	27	46	30,09	26		NNE	1	Cloudy and hazy.
	2	0	35	51	30,08	35		SE	1,2	Fine.
19	8	0	33	46	30,10	32		N	1	Cloudy.
	2	0	34	49	30,08	36		SSE	1.2	Cloudy.
20	8	0	32	45	29,91	31		WSW	1	Snow.
	2	0	33	43	29,86	33		W	1	Cloudy.
21	8	0	30	42	29,83	30		S	1	Cloudy.
	2	0	36	50	29,83	38		N	1	Rain.
22	8	0	36	46	29,81	35		N	1	Rain.
	2	0	48	48	29,59	37		S	1	Cloudy.
23	8	0	46	48	29,57	45		SSE	1	Cloudy.
	2	0	47	52	29,52	49		SSE	1	Rain.
24	8	0	38	48	29,45	37		SW	1	Cloudy.
	2	0	39	53	29,38	39		N	1	Rain
25	8	0	36	49	29,44	36		NbyE	2.3	Rain.
	2	0	39	52	29,52	39		N	1	Cloudy.
26	8	0	35	48	29,93	35		E	1	Cloudy.
	2	0	35	54	30,02	37		N	1	Cloudy.
27	8	0	32	46	30,11	31		N	1	Cloudy.
	2	0	39	47	30,09	43		N	1	Fine.
28	8	0	32	43	30,00	30		E	1	Fine.
	2	0	40	48	29,94	48		E	1	Cloudy.
29	8	0	28	44	29,87	28		SW	1	Cloudy.
	2	0	42	49	29,74	48		W	1	Fine.

METEOROLOGICAL JOURNAL
for March, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Mar. 1	7	0	39	47	29,50	37	Rain this Month 1,325 Inches.	N	1	Cloudy.
	2	0	45	53	29,57	55		N	1	Cloudy.
2	7	0	34	46	28,95	34		N	2.3	Snow.
	2	0	41	55	29,17	42		E	2	Fine.
3	7	0	31	44	29,70	29		N	1.2	Fine.
	2	0	43	54	29,84	43		N	1	Fine.
4	7	0	31	45	29,99	30		N	1	Fine.
	2	0	38	50	29,99	40		SE	1	Fine.
5	7	0	28	43	30,16	31		NE	1	Fine.
	2	0	41	43	30,16	42		NE	1	Fine.
6	7	0	31	43	30,16	30		N	1	Cloudy.
	2	0	36	47	30,15	39		NE	1	Cloudy.
7	7	0	31	43	30,17	28		N	1	Cloudy.
	2	0	34	50	30,12	35		W	1	Rain.
8	7	0	34	44	30,24	34		NbyE	1	Cloudy.
	2	0	41	49	30,25	45		E	1	Cloudy.
9	7	0	33	44	30,18	30		W	1	Fine.
	2	0	44	51	30,06	47		W	1	Cloudy.
10	7	0	35	47	29,87	33		S	1	Cloudy.
	2	0	42	52	29,71	46		SSE	1	Fine.
11	7	0	35	47	29,64	33		E	1	Cloudy and hazy.
	2	0	45	57	29,58	56		E	1	Fine.
12	7	0	34	49	29,47	33		E	1	Cloudy.
	2	0	47	53	29,41	57		E	1	Fine.
13	7	0	40	48	29,57	38		NW	1.2	Cloudy.
	2	0	48	55	29,72	50		NW	1	Fine.
14	7	0	43	49	30,07	38		W	2	Cloudy.
	2	0	52	57	30,15	53		SSW	1	Cloudy.
15	7	0	49	53	30,23	48		NW	1	Cloudy.
	2	0	58	57	30,25	61		N	1	Cloudy.
16	7	0	46	54	30,36	46		E	1	Cloudy.
	2	0	48	58	30,36	51		E	1	Fine.

METEOROLOGICAL JOURNAL
for March, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Mar. 17	7	0	40	54	30,27	39	Rain this Month 1,325 Inches.	SW	1	Cloudy, and very hazy.
	2	0	50	60	30,25	50		N	1	Fine.
18	7	0	38	52	30,34	37		SE	1	Fine.
	2	0	54	60	30,33	55		E	1	Fine. [night.
19	7	0	41	52	30,25	40		N	1	Cloudy, some rain in the
	2	0	43	55	30,22	47		E	1	Cloudy.
20	7	0	38	49	30,24	38		N	1	Cloudy and hazy.
	2	0	44	56	30,22	46		SW	1	Fine.
21	7	0	36	49	30,16	35		W	1	Cloudy and hazy.
	2	0	49	55	30,02	50		W	1	Cloudy.
22	7	0	44	51	29,76	39		N	1	Fine.
	2	0	50	63	29,80	52		NW	1	Fine.
23	7	0	45	53	29,36	44		W	1.2	Fine, rain in the night.
	2	0	48	60	29,11	53		WNW	2.3	Cloudy.
24	7	0	43	54	29,07	40		W	1	Fine.
	2	0	48	60	28,93	54		S	1	Fine.
25	7	0	38	51	29,17	37		NW	1	Fine.
	2	0	46	63	29,36	46		S	1	Fine.
26	7	0	36	51	29,76	33		SSW	1	Cloudy.
	2	0	43	53	29,58	43		SSW	1	Cloudy, rain at 1 P. M.
27	7	0	37	52	29,77	38		W	1	Rain.
	2	0	51	62	29,80	52		SbyW	1	Cloudy.
28	7	0	47	53	29,95	45		W	1.2	Fine.
	2	0	57	63	30,01	60		SW	1	Fine.
29	7	0	50	56	30,17	48		SW	1	Fine.
	2	0	62	64	30,07	61		SW	1	Fine.
30	7	0	49	54	29,94	48		SW	1	Fine.
	2	0	64	63	29,96	64		N	1	Fine.
31	7	0	45	55	30,01	41		SW	1	Cloudy.
	2	0	60	67	29,98	66		S	1	Fine.

METEOROLOGICAL JOURNAL

for April, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Apr. 1	7	0	42	54	30,01	41	Rain this Month 1,514 Inches.	W	2	Cloudy.
	2	0	55	58	29,97	60		W	1	Cloudy.
2	7	0	52	56	30,06	47		W	1	Cloudy.
	2	0	56	59	30,06	64		W	1	Fine.
3	7	0	53	58	30,16	52		NE	1	Cloudy.
	2	0	58	61	30,17	59		E	1	Cloudy.
4	7	0	47	55	29,98	42		SE	1.2	Fine and clear.
	2	0	64	64	29,88	66		E	1	Fine.
5	7	0	48	57	29,80	45		NE	1	Hazy thick weather.
	2	0	62	66	29,71	66		S	1	Fine.
6	7	0	48	57	29,40	45		SE	1	Rain.
	2	0	54	60	29,38	56		W	1	Rain.
7	7	0	37	53	29,43	36		W	1	Fine.
	2	0	55	61	29,47	55		SW	1	Cloudy.
8	7	0	41	53	29,38	37		S	1.2	Fine.
	2	0	50	61	29,27	52		SbyE	1.2	Cloudy.
9	7	0	46	53	29,23	45		SSW	1	Cloudy.
	2	0	53	56	29,36	54		NW	1	Cloudy.
10	7	0	44	53	29,49	38		SSE	2	Cloudy.
	2	0	47	60	29,38	48		S	2	Rain.
11	7	0	47	53	29,44	46		SW	1	Cloudy.
	2	0	53	62	29,54	54		SW	1	Fine.
12	7	0	48	55	29,71	47		SW	1	Cloudy and hazy.
	2	0	56	61	29,79	56		S	1	Cloudy.
13	7	0	49	55	29,86	48		WSW	1	Cloudy and hazy.
	2	0	50	61	29,82	52		N	1	Rain.
14	7	0	49	57	29,59	47		SSW	1	Cloudy and hazy.
	2	0	50	61	29,59	52		WNW	1	Rain.
15	7	0	45	55	29,75	42		WNW	1	Fine.
	2	0	51	62	29,81	54		WNW	1	Fine.

METEOROLOGICAL JOURNAL

for April, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Apl. 16	7	0	47	55	30,04	40	Rain this Month 1,514 Inches.	WNW	1	Fine.
	2	0	58	60	30,06	58		WNW	1	Fine.
17	7	0	55	56	30,19	44		W	1	Fair.
	2	0	62	62	30,18	67		N	1	Fair.
18	7	0	51	58	30,16	58		N	1	Cloudy and hazy.
	2	0	60	65	30,13	64		SSE	1	Fine.
19	7	0	56	59	30,13	50		N	1	Fine.
	2	0	63	64	30,09	66		N	1	Fine.
20	7	0	53	59	30,11	49		NW	1	Fine.
	2	0	67	65	30,18	67		N	1	Cloudy.
21	7	0	51	59	30,27	48		NW	1	Fine.
	2	0	61	67	30,25	65		N	1	Fine.
22	7	0	53	59	30,33	48		E	1	Fine.
	2	0	59	63	30,34	60		E	1	Fine.
23	7	0	51	57	30,46	45		ESE	1	Fine.
	2	0	60	63	30,47	60		ESE	1	Fine.
24	7	0	50	58	30,50	44		NE	1	Fine.
	2	0	62	66	30,47	65		E	1.2	Fine.
25	7	0	48	56	30,42	44		N	1	Cloudy.
	2	0	60	64	30,31	61		N	1	Fine.
26	7	0	52	55	29,97	45		WNW	1	Fine.
	2	0	57	60	29,78	68		WSW	1	Fine.
27	7	0	45	55	29,67	45		N	2	Cloudy, rain in the night.
	2	0	45	56	29,81	49		NNW	1	Cloudy.
28	7	0	43	55	29,97	41		SSW	1	Cloudy.
	2	0	51	60	30,02	55		W	1	Fine.
29	7	0	48	55	30,08	40		W	1	Fine.
	2	0	55	59	30,08	63		W	1	Cloudy.
30	7	0	44	53	30,15	49		NW	1	Rain.
	2	0	56	58	30,19	69		WNW	1	Fine.

METEOROLOGICAL JOURNAL

for May, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
May 1	7	0	47	52	30,28	42	Rain this Month 1,393 Inches.	SbW	1	Fine.
	2	0	54	57	30,29	67		SSW	1	Fine.
2	7	0	47	50	30,25	45		W	1	Cloudy.
	2	0	56	58	30,15	60		E	1	Cloudy.
3	7	0	45	56	30,14	45		ESE	1	Cloudy and hazy.
	2	0	49	56	30,10	50		E	1	Cloudy and hazy.
4	7	0	45	56	29,98	43½		E	1	Cloudy and hazy.
	2	0	53	59	29,88	57		E	1	Cloudy.
5	7	0	43	54	29,85	39		NE	1	Cloudy.
	2	0	52	60	29,84	56		N	1	Fine.
6	7	0	45	54	29,88	40		W	1	Fine.
	2	0	55	60	29,81	60		W	1	Fine.
7	7	0	50	52	29,58	48		WSW	1	Cloudy.
	2	0	55	57	29,54	60		WSW	1	Fine.
8	7	0	54	56	29,68	51		SW	1	Cloudy, dull weather.
	2	0	60	58	29,66	66		WSW	1	Cloudy.
9	7	0	53	60	29,62	51		S	1	Cloudy, rain in the night.
	2	0	60	64	29,66	68		WSW	1	Fine.
10	7	0	55	59	29,78	53		S	2	Fine.
	2	0	63	62	29,83	64		S	2	Cloudy.
11	7	0	54	59	29,92	52		S	2.3	Fine, rather cloudy.
	2	0	61	66	29,95	61		S	1	Fine.
12	7	0	54	60	30,00	49		W	1	Cloudy.
	2	0	62	63	30,03	65		W	1	Fine.
13	7	0	53	60	29,96	49		E	1	Hazy.
	2	0	60	60	29,87	60		SSW	1	Cloudy.
14	7	0	55	58	29,86	51		SSW	1	Fine.
	2	0	59	61	29,81	63		SSW	1	Fine.
15	7	0	52	58	29,79	48		S	1	Fair.
	2	0	63	64	29,79	65		S	1	Fine.
16	7	0	53	60	29,68	51		W	1	Cloudy, rain in the night.
	2	0	58	63	29,66	63		SSW	1	Fine.

METEOROLOGICAL JOURNAL

for May, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
May 17	7	0	53	59	29,74	49	Rain this Month 1,393 Inches.	SW	1.2	Cloudy.
	2	0	61	65	29,74	66		SW	1	Fine.
18	7	0	48	60	29,33	47		SW	1.2	Rain.
	2	0	54	62	29,17	56		S	3	Rain.
19	7	0	54	59	29,64	47		SW	1.2	Fine.
	2	0	60	62	29,79	70		WSW	1	Cloudy.
20	7	0	53	60	30,13	49		SW	1	Cloudy.
	2	0	60	62	30,21	65		SW	1	Cloudy.
21	7	0	54	60	30,22	49		SW	1	Fine.
	2	0	59	63	30,31	68		WSW	1	Fine.
22	7	0	54	60	30,20	52		E	1	Fine.
	2	0	68	68	30,14	68		E	1	Fine.
23	7	0	59	62	29,99	55		E	1	Fine.
	2	0	67	71	29,94	75		E	1	Fine.
24	7	0	62	64	29,71	56		E	1	Fine.
	2	0	67	67	29,76	69		W	1	Fair.
25	7	0	54	61	29,74	51		S	2	Rain.
	2	0	56	63	29,74	60		W	1.2	Rain.
26	7	0	52	59	29,80	47		W	1	Cloudy.
	2	0	56	62	29,78	62		S	2	Cloudy.
27	7	0	53	61	29,66	53		W	1	Rain.
	2	0	57	62	29,62	64		W	1	Fine.
28	7	0	53	59	29,68	50		NW	1	Cloudy.
	2	0	55	60	29,57	58		W	1	Rain.
29	7	0	53	57	29,35	43		NW	1	Cloudy.
	2	0	54	58	29,33	60		SW	1	Rain.
30	7	0	49	57	29,31	44		W	1	Fine.
	2	0	60	60	29,33	60		SSW	1	Fine.
31	7	0	52	56	29,34	47		W	1	Fine.
	2	0	57	61	29,39	59		N	1	Fine.

METEOROLOGICAL JOURNAL

for June, 1820.

1820	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
June 1	7 0	50	55	29,54	45	Rain this Month 2,331 Inches.	W	2	Fine.
	2 0	59	61	29,54	59		WNW	1	Fine.
2	7 0	52	57	29,55	49		SW	1	Fine.
	2 0	57	60	29,57	59		W	1	Rain.
3	7 0	50	57	29,71	46		WbN	1	Cloudy.
	2 0	60	60	29,73	60		NW	1	Cloudy.
4	7 0	50	56	29,86	49		NW	1	Rain.
	2 0	56	57	29,88	57		NW	1	Cloudy.
5	7 0	51	56	29,95	45		N	1	Fine.
	2 0	63	59	29,94	63		W	1	Cloudy.
6	7 0	52	57	29,83	51		W	1	Cloudy.
	2 0	58	57	29,97	59		W	1	Cloudy.
7	7 0	50	56	30,06	47		W	1	Fine.
	2 0	61	59	29,95	63		N	1	Cloudy.
8	7 0	54	57	29,94	54		N	1	Cloudy.
	2 0	64	59	29,92	64		WbN	1	Cloudy.
9	7 0	55	59	29,80	54		W	1	Rain.
	2 0	61	60	29,73	63		NW	1	Cloudy.
10	7 0	50	57	29,72	45		WbN	1	Fine.
	2 0	57	58	29,72	57		WbN	1	Fine.
11	7 0	51	57	29,61	45		WNW	1	Fine.
	2 0	52	56	29,55	53		WNW	1	Cloudy.
12	7 0	52	56	29,69	49		NW	1	Rain.
	2 0	60	60	29,77	65		E	1	Cloudy.
13	7 0	50	56	29,85	45		NW	1	Fine.
	2 0	58	59	29,85	63		NW	1	Cloudy, some rain
14	7 0	52	59	30,06	49		SE	1	Cloudy.
	2 0	57	58	30,05	59		SE	1	Fine.
15	7 0	52	57	29,81	49		NW	1	Rain.
	2 0	57	58	29,89	63		NW	1	Cloudy.

METEOROLOGICAL JOURNAL

for June, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
June 16	7	0	53	56	29,94	48	Rain this Month 2,331 Inches.	N	1	Cloudy.
	2	0	59	58	29,94	60		NW	1	Cloudy.
17	7	0	55	57	29,92	53		NNW	1	Cloudy.
	2	0	63	60	29,95	66		W	1	Cloudy.
18	7	0	57	57	30,06	52		W	1	Fine.
	2	0	64	62	30,01	64		NNE	1	Fine.
19	7	0	57	59	29,70	52		N	1	Cloudy, rain in the night.
	2	0	63	63	29,72	65		NW	1	Fine.
20	7	0	53	59	29,64	51		W	1	Cloudy.
	2	0	60	60	29,75	66		NW	1	Rain.
21	7	0	54	59	29,85	50		NW	1	Fine.
	2	0	64	61	29,90	65		N	1	Cloudy.
22	7	0	56	59	30,02	52		NW	1	Cloudy.
	2	0	67	62	30,02	70		W	1	Cloudy.
23	7	0	58	60	30,11	54		W	1	Fine.
	2	0	60	61	30,13	65		W	1	Fine.
24	7	0	61	63	30,18	58		SW	1	Fine.
	2	0	72	73	30,21	76		SSW	1	Fine.
25	7	0	67	67	30,32	63		N	1	Fine.
	2	0	75	72	30,33	80		SSW	1	Fair.
26	7	0	70	70	30,38	65		W	1	Fine.
	2	0	84	77	30,37	84		NE	1	Fine.
27	7	0	71	70	30,37	70		E	1	Fine.
	2	0	79	76	30,37	85		WSW	1	Fine.
28	7	0	70	72	30,31	66		S	1	Fine.
	2	0	82	79	30,25	84		NW	1	Fine.
29	7	0	62	69	30,18	60		E	1	Cloudy.
	2	0	72	76	30,09	85		E	1	Fine.
30	7	0	62	69	29,94	58		E	1	Cloudy and hazy.
	2	0	67	67	29,98	69		E	1	Cloudy.

METEOROLOGICAL JOURNAL

for July, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
July 1	7	0	59	64	30,18	58	Rain this Month 2,807 Inches.	N	1	Fine, rather cloudy.
	2	0	64	65	30,22	66		N	1	Fine.
2	7	0	56	64	30,17	50		W	1	Fine.
	2	0	60	63	30,05	65		W	1	Rain.
3	7	0	54	62	29,96	51		NW	1	Cloudy, rather hazy.
	2	0	62	64	29,91	64		W	1	Cloudy.
4	7	0	57	61	29,92	54		NNW	1	Cloudy.
	2	0	60	61	29,98	63		E	1	Cloudy.
5	7	0	55	61	30,07	54		N	1	Fine.
	2	0	60	60	30,09	61		NbE	1	Cloudy.
6	7	0	54	60	30,11	50		NE	1	Cloudy.
	2	0	63	62	30,12	64		NbE	1	Cloudy.
7	7	0	57	61	30,11	54		NbE	1	Cloudy.
	2	0	63	63	30,12	65		N	1	Fine.
8	7	0	56	61	30,15	55		NE	1	Cloudy, rather hazy.
	2	0	60	60	30,16	64		N	1	Cloudy.
9	7	0	56	60	30,16	54		NbE	1	Cloudy.
	2	0	65	60	30,14	64		NE	1	Cloudy.
10	7	0	58	60	30,12	58		E	1	Cloudy.
	2	0	66	65	30,08	68		E	1	Fine, rather cloudy.
11	7	0	56	61	30,06	53		EbS	1	Fine.
	2	0	66	64	30,02	67		ESE	1	Fine.
12	7	0	53	60	29,97	52		E	1	Fine.
	2	0	65	65	29,88	68		E	1	Cloudy.
13	7	0	59	61	29,86	56		EbS	1	Cloudy.
	2	0	62	61	29,82	65		ESE	1	Cloudy.
14	7	0	59	61	29,82	55		NNE	1	Cloudy.
	2	0	65	62	29,84	68		NE	1	Fine.
15	7	0	62	63	29,90	60		SW	1	Cloudy.
	2	0	70	65	29,93	73		WSW	1	Fine.
16	7	0	60	63	29,95	57		E	1	Fine. [der storm at 11 A.M.
	2	0	72	67	29,90	74		SE	1	Fine, rather cloudy, a thun-

METEOROLOGICAL JOURNAL

for July, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
July 17	7	0	62	66	29,64	60	Rain this Month 2,807 Inches.	E	1	Rain.
	2	0	68	65	29,46	70		W	1	Cloudy, rain from 11 till 2.
18	7	0	55	63	29,39	55		S	1	Rains hard.
	2	0	67	65	29,35	69		WSW	1	Cloudy.
19	7	0	56	63	29,42	55		W	1	Cloudy, rather hazy.
	2	0	68	66	29,52	70		W	1	Cloudy.
20	7	0	58	63	29,65	56		W	1	Fine.
	2	0	69	70	29,71	70		WNW	1	Fine.
21	7	0	61	64	29,84	59		SW	1	Cloudy.
	2	0	70	66	29,91	71		W	1	Fine.
22	7	0	60	62	29,93	56		WSW	1	Fine.
	2	0	62	64	29,91	70		WSW	1	Cloudy.
23	7	0	57	63	29,89	59		WbN	1	Cloudy.
	2	0	65	63	29,94	66		NW	1	Cloudy.
24	7	0	58	63	29,95	55		W	1	Cloudy.
	2	0	69	66	29,95	70		NW	1	Fine.
25	7	0	62	65	29,86	60		W	1	Fine.
	2	0	67	67	29,84	70		W	1	Fine.
26	7	0	56	63	29,98	54		W	1	Fine.
	2	0	69	67	30,04	70		WNW	1	Fine.
27	7	0	62	65	30,04	60		W	1	Cloudy.
	2	0	72	72	30,02	72		WSW	1	Fine.
28	7	0	62	65	30,00	58		W	1	Fine.
	2	0	71	69	30,02	72		SW	1	Fine.
29	7	0	64	67	30,06	62		E	1	Cloudy.
	2	0	73	72	30,07	75		SSW	1	Fine.
30	7	0	63	67	30,06	61		SW	1	Fine.
	2	0	75	71	30,02	77		SE	1	Cloudy.
31	7	0	64	69	29,83	62		E	1	Fine.
	2	0	75	77	29,83	80		W	1	Fine.

METEOROLOGICAL JOURNAL
for August, 1820.

1820	Time. H.	M	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Aug. 1	7	0	53	71	29,78	46	Rain this Month 1,315 Inches.	W	1	Cloudy.
	2	0	73	70	29,83	75		SSW	1	Cloudy.
2	7	0	57	60	30,04	56		E	1	Fine.
	2	0	71	70	30,08	75		WSW	1	Fine.
3	7	0	64	68	29,98	60		S	1,2	Cloudy.
	2	0	71	69	29,89	75		S	1,2	Cloudy.
4	7	0	64	68	29,72	63		S	1	Cloudy, rain in the night.
	2	0	72	71	29,74	75		SWbS	1	Fine.
5	7	0	59	67	29,79	57		W	1	Fine.
	2	0	69	70	29,82	72		WSW	1	Cloudy. [in the night
6	7	0	57	65	29,72	56		S	2	Rain, much rain and wind
	2	0	68	67	29,58	70		WSW	1,2	Cloudy.
7	7	0	60	64	29,68	57		W	1	Cloudy.
	2	0	69	69	29,87	70		WbN	1	Cloudy.
8	7	0	54	63	29,96	58		WSW	1	Cloudy.
	2	0	69	65	29,99	70		WSW	1	Cloudy.
9	7	0	57½	64	29,91	60		ESE	1	Fine.
	2	0	69	71	30,04	70		WNW	1	Fine.
10	7	0	59	65	30,25	55		W	1	Cloudy.
	2	0	71	72	30,26	72		S	1	Fine.
11	7	0	61	66	30,25	59		W	1	Hazy.
	2	0	72	73	30,22	72		W	1	Fine.
12	7	0	60	66	30,16	62		W	1	Cloudy.
	2	0	71	72	30,15	77		W	1	Fine.
13	7	0	61	67	30,11	59		N	1	Fine, rather cloudy.
	2	0	70	72	30,06	71		NW	1	Fine.
14	7	0	61	67	29,98	59		SbW	1	Fine.
	2	0	72	75	29,94	75		SSW	1	Fine.
15	7	0	60	67	29,88	57		W	1	Fine.
	2	0	73	73	29,82	75		WSW	1	Fine.
16	7	0	64	69	29,78	62		W	1	Cloudy.
	2	0	72	71	29,81	74		W	1,2	Cloudy.

METEOROLOGICAL JOURNAL

for August, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Aug. 17	7	0	66	68	29,79	63		SW	2	Cloudy.
	2	0	71	71	29,76	73		W	2	Fair.
18	7	0	61	68	29,83	59		N	1	Hazy and cloudy.
	2	0	73	69	29,81	74		WSW	1	Fine.
19	7	0	59	66	29,72	57		E	1	Rather cloudy.
	2	0	68	66	29,71	70		WSW	1	Cloudy.
20	7	0	53	64	29,84	50		W	1	Fine.
	2	0	64	72	29,85	67		SSW	1	Cloudy.
21	7	0	58	63	29,82	56		N	1	Rain.
	2	0	58	64½	29,77	65		ESE	1	Rain.
22	7	0	54	62	29,76	53		N	2	Cloudy, rain in the night.
	2	0	59	62	29,79	60		WSW	2	Cloudy.
23	7	0	52	59	29,99	50	Rain this Month 1,315 Inches.	N	1	Fine, rather hazy.
	2	0	60	61	30,09	62		N	1	Cloudy.
24	7	0	56	60	30,15	52		E	1	Fine.
	2	0	63	64	30,08	66		WSW	1	Fine.
25	7	0	57	60	29,91	52		S	1.2	Cloudy.
	2	0	65	62	29,80	66		SW	1.2	Cloudy.
26	7	0	57	61	29,67	55		SW	2	Fine.
	2	0	64	65	29,66	68		W	1	Cloudy.
27	7	0	53	61	29,66	51		W	1	Fine.
	2	0	64	64	29,72	66		WSW	1	Rain.
28	7	0	54	62	29,48	53		WbN	1	Fine and clear.
	2	0	60	64	29,51	65		SW	1	Fine.
29	7	0	52	60	29,61	50		W	1	Fine.
	2	0	64	66	29,69	66		ESE	1	Fair.
30	7	0	51	61	29,86	49		WbN	1	Fine, but rather hazy.
	2	0	61	64	29,99	64		SSW	1	Fine.
31	7	0	53	61	30,02	51		EbS	1	Cloudy and hazy.
	2	0	61	65	30,04	63		NE	1	Fair.

METEOROLOGICAL JOURNAL

for September, 1820.

1820	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Sep. 1	7 0	53	60	30,07	51	Rain this Month 2,058 Inches.	NE	1	Fine, rather hazy.
	2 0	64	63	30,05	65		NE	1	Fine.
2	7 0	54	60	30,03	52		N	1	Fine, rather hazy.
	2 0	63	64	30,03	64		ESE	1	Fine.
3	7 0	57	61	30,08	55		N	1	Cloudy.
	2 0	64	64	30,11	64		SSE	1	Fine, rather hazy.
4	7 0	50	60	30,14	48		SW	1	Cloudy and hazy.
	2 0	65	64	30,14	66		N	1	Cloudy.
5	7 0	52	60	30,10	50		NbyE	1	Hazy.
	2 0	55	65	30,07	56		ENE	1	Fine.
6	7 0	53	61	30,03	51		NE	1	Cloudy, rather hazy.
	2 0	63	68	30,03	64		E	1	Fine.
7	7 0	54	61	30,07	51		E	1	Hazy.
	2 0	62	66	30,07	64		SE	1	Fine.
8	7 0	57	63	30,17	55		W	1	Cloudy and hazy.
	2 0	67	66	30,25	67		N	1	Fine.
9	7 0	53	63	30,34	52		E	1	Hazy.
	2 0	65	69	30,35	68		WNW	1	Fine.
10	7 0	54	63	30,30	52		SW	1	Cloudy.
	2 0	66½	69	30,27	67		WNW	1	Fair.
11	7 0	57	64	30,26	56		N	1	Hazy thick weather.
	2 0	69½	70	30,25	72		ESE	1	Cloudy.
12	7 0	61	66	30,21	59		E	1	Hazy.
	2 0	70	73	30,22	73		ESE	1	Fine.
13	7 0	58	66	30,17	56		E	1	Hazy.
	2 0	69	74	30,13	70		E	1	Fine.
14	7 0	59	66	29,96	56		S	1	Fine.
	2 0	71	72	29,88	72		SSE	1	Fine, rather hazy.
15	7 0	60	67	29,69	58		SW	1.2	Fine.
	2 0	63	67	29,76	71		WSW	1	Fine, rather hazy.

METEOROLOGICAL JOURNAL

for September, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Sep. 16	7	0	55	62	29,94	51	Rain this Month 2,058 Inches.	W	1	Fine.
	2	0	64	64	29,96	65		W	1	Fine.
17	7	0	57	63	29,97	54		W	1	Fine.
	2	0	62	63	29,91	65		W	1	Fine.
18	7	0	54	62	29,53	53		N	1	Rain ; a heavy rain in the night.
	2	0	56	61	29,59	66		N	1.2	Cloudy.
19	7	0	46	67	29,80	43		WNW	1	Fine.
	2	0	54	63	29,94	56		WbN	1	Fair.
20	7	0	42	57	29,98	40		W	1	Fine.
	2	0	55	59	29,75	58		SW	1	Rain.
21	7	0	44	56	29,44	44		W	1	Fine.
	2	0	54	59	29,41	57		NW	1.2	Cloudy.
22	7	0	47	55	29,64	43		N	1	Cloudy, rather hazy.
	2	0	56	58	29,82	59		WNW	1	Cloudy.
23	7	0	56	57	29,96	47		SW	1	Cloudy.
	2	0	64	60	29,97	65		W	1	Fine.
24	7	0	57	59	29,75	56		SSW	2	Rain.
	2	0	65	61	29,58	66		W	1	Fine.
25	7	0	51	57	29,66	49		W	1	Fine.
	2	0	53	62	29,65	64		NW	1	Fine.
26	7	0	48	56	29,83	44		N	1	Cloudy.
	2	0	53	58	29,99	56		NNE	1	Cloudy.
27	7	0	41	53	30,12	38		W	1	Cloudy.
	2	0	53	56	30,04	55		W	1	Rain.
28	7	0	48	54	30,07	41		SSW	1	Cloudy.
	2	0	59	58	30,03	60		W	1	Cloudy.
29	7	0	53	57	29,95	48		NE	1	Rain.
	2	0	57	56	30,11	61		SSE	1	Cloudy.
30	7	0	48	56	30,16	46		E	1	Cloudy and hazy.
	2	0	61	58	30,05	62		S	1	Cloudy.

METEOROLOGICAL JOURNAL

for October, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Oct. 1	8	0	52	57	30,07	48	Rain this Month 1,582 Inches.	NNE	1	Cloudy.
	2	0	57	61	30,21	62		N	1	Cloudy.
2	8	0	49	56	30,36	46½		W	1	Fine, rather hazy.
	2	0	57	59	30,42	58		NNW	1	Cloudy.
3	8	0	50	56	30,49	46		NbE	1	Fine, rather hazy.
	2	0	56	60	30,53	58		NNE	1	Fine.
4	8	0	46	55	30,52	43		NE	1	Fine.
	2	0	56	57	30,44	58		ESE	1	Fine.
5	8	0	51	56	30,34	46		N	1	Fine, rather hazy.
	2	0	56	61	30,27	58		NE	1	Fine.
6	8	0	51	56	30,23	48		NEbN	1	Cloudy.
	2	0	58	60	30,19	59		E	1	Fine.
7	8	0	49	57	30,16	48		N	1	Fine, rather hazy.
	2	0	58	61	30,14	60		ESE	1	Cloudy.
8	8	0	49	56	30,14	48		EbN	1	Cloudy.
	2	0	58	58	30,17	60		E	1	Cloudy.
9	8	0	50	56	30,20	49		N	1	Cloudy.
	2	0	55	57	30,19	58		NNW	1	Cloudy.
10	8	0	47	55	30,13	47		N	1	Cloudy.
	2	0	64	56	30,07	65		SW	1	Cloudy.
11	8	0	47	55	29,99	46		N	1	Fine.
	2	0	52	53	29,95	54		N	1	Cloudy.
12	8	0	45	49	30,01	44		N	1	Fine.
	2	0	50	56	30,04	52		N	1	Cloudy.
13	8	0	39	51	29,99	39		W	1	Fine, rather hazy.
	2	0	50	53	29,95	52		E	1	Cloudy.
14	8	0	47	52	29,75	40		SE	1	Cloudy.
	2	0	52	49	29,40	54		SbE	1	Cloudy. [hard in the night.
15	8	0	58	56	29,03	50		S	3	Stormy ; rained & blowed
	2	0	57	58	29,01	62		S	1	Cloudy.
16	8	0	47	54	29,15	46		W	1	Cloudy.
	2	0	51	55	29,13	56		SW	1	Fine.

METEOROLOGICAL JOURNAL

for October, 1820.

1820	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Oct. 17	8 0	48	53	28,84	45	Rain this Month 1,582 Inches.	S	2	Rain.
	2 0	52	57	28,84	55		W	2	Fine.
18	8 0	43	54	29,93	43		W	1	Cloudy, rather hazy.
	2 0	52	57	29,98	54		W	1	Rain.
19	8 0	46	54	30,26	46		W	1	Cloudy.
	2 0	53	55	29,31	55		SSW	1	Cloudy.
20	8 0	43	53	28,86	43		WSW	1.2	Fine.
	2 0	49	55	29,92	54		WSW	1	Cloudy.
21	8 0	40	52	29,33	41		W	1	Hazy.
	2 0	51	55	29,49	53		N	1	Fine.
22	8 0	40	52	29,09	40		S	3	A violent gale of wind and [rain.
	2 0	50	54	28,71	52		S	1.2	Cloudy.
23	8 0	46	52	28,97	45		N	1	Cloudy.
	2 0	51	57	29,13	53		NNW	1	Cloudy.
24	8 0	45	53	28,88	45		SW	1	Fine.
	2 0	48	56	28,69	52		WSW	1	Cloudy.
25	8 0	44	55	28,96	42		WSW	1	Cloudy and hazy.
	2 0	51	58	29,20	53			1	Cloudy.
26	8 0	42	53	29,38	41		S	1	Cloudy.
	2 0	49	57	29,03	52		SSW	1	Cloudy.
27	8 0	47	54	28,96	41		W	1	Fine.
	2 0	52	57	29,11	57		W	1	Cloudy.
28	8 0	43	55	29,54	41		W	1	Fine.
	2 0	51	59	29,66	52		NW	1	Fine.
29	8 0	42	53	29,54	41		SbyE	1	Cloudy.
	2 0	46	53	29,27	52		SSE	1	Rain.
30	8 0	41	53	29,51	40		W	1	Cloudy.
	2 0	48	62	29,62	50		WSW	1	Cloudy.
31	8 0	44	55	29,49	41		WSW	1	Cloudy and hazy.
	2 0	49	58	29,35	50		ESE	1	Cloudy.

METEOROLOGICAL JOURNAL

for November, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Nov. 1	8	0	45	56	29,33	44	Rain this Month 1,000 Inches.	N	1	Cloudy and dark.
	2	0	47	58	29,46	49		N	1	Cloudy.
2	8	0	37	54	29,73	37		W	1	Fine.
	2	0	47	60	29,75	48		WNW	1	Fine.
3	8	0	34	46	29,82	34		W	1	Foggy.
	2	0	46	56	29,86	47		SW	1	Fine.
4	8	0	36	49	29,85	35		N	1	Foggy.
	2	0	47	55	29,86	49		NE	1	Fine.
5	8	0	31	49	29,93	31		W	1	Hazy.
	2	0	42	49	29,85	47		ESE	1	Rain.
6	8	0	47	51	29,69	31		W	1	Cloudy and hazy.
	2	0	52	57	29,74	53		W	1	Cloudy.
7	8	0	50	54	29,74	47		SW	1	Cloudy.
	2	0	55	58	29,74	56		S	1	Fine.
8	8	0	51	57	29,79	51		SE	1	Cloudy and hazy.
	2	0	54	57	29,81	57		ESE	1	Rain.
9	8	0	47	56	29,89	47		E	1	Cloudy.
	2	0	49	58	29,93	54		ESE	1	Cloudy and hazy.
10	8	0	44	57	29,97	44		N	1	Cloudy.
	2	0	48	58	30,10	52			1	Fine.
11	8	0	44	56	30,11	43		NbE	1	Cloudy.
	2	0	48	58	30,19	50		SSW	1	Cloudy.
12	8	0	39	53	30,13	39		NW	1	Cloudy.
	2	0	45	57	29,95	49			1	Cloudy.
13	8	0	39	51	29,57	38		NW	1	Cloudy.
	2	0	40	54	29,52	41			1	Cloudy.
14	8	0	35	51	29,76	35		N	1	Hazy.
	2	0	39	52	29,77	39		SSW	1	Cloudy.
15	8	0	35	49	29,87	38		NbE	1	Fine.
	2	0	40	52	29,91	43		NE	1	Fine.

METEOROLOGICAL JOURNAL

for November, 1820.

1820	Time.		Therm. without.	Therm. within.	Barom.	Six's Therm.	Rain this Month 1,000 Inches.	Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Nov. 16	8	0	36	47	29,92	35		NbW	1	Fine, rather hazy.
	2	0	40	52	29,85	42		NbW	1	Cloudy and hazy.
17	8	0	35	50	29,67	32		SE	1	Cloudy and hazy.
	2	0	43	56	29,63	44			1	Rain.
18	8	0	36	51	29,80	35		NW	1	Hazy.
	2	0	45	55	29,86	47		WSW	1	Cloudy and hazy.
19	8	0	37	50	29,91	35			1	Foggy.
	2	0	45	50	29,91	47		WSW	1	Cloudy.
20	8	0	39	51	29,95	38		SbE	1	Fine.
	2	0	49	53	29,93	50		S	1.2	Cloudy.
21	8	0	48	52	29,88	47		SSW	1.2	Cloudy.
	2	0	49	57	29,83	51		SSW	1	Cloudy and hazy.
22	8	0	46	53	29,77	46		SbyE	1	Rain.
	2	0	49	58	29,74	50		SbyE	1	Rain.
23	8	0	44	54	29,57	44		E	1	Fine.
	2	0	47	61	29,50	50		NE	1	Cloudy.
24	8	0	40	57	29,68	39			1	Thick fog.
	2	0	48	59	29,70	49		NE	1	Cloudy.
25	8	0	43	55	29,56	40		E	1	Cloudy.
	2	0	47	57	29,56	48		ESE	1	Cloudy.
26	8	0	45	55	29,78	47		S	1	Fine.
	2	0	42	55	29,84	44		SSW	1	Cloudy.
27	8	0	42	53	29,89	42		E	1	Cloudy.
	2	0	48	57	29,90	52		E	1	Fine.
28	8	0	40	52	30,05	38		NE	1	Cloudy.
	2	0	43	57	30,11	47		SSE	1	Cloudy and hazy.
29	8	0	39	53	30,26	39		E	1	Cloudy.
	2	0	44	57	30,27	44		E	1	Cloudy.
30	8	0	42	52	30,23	42		NE	1	Cloudy.
	2	0	41	55	30,19	44		WNW	1	Cloudy.

METEOROLOGICAL JOURNAL
for December, 1820.

1820	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Dec. 1	8 0	38	52	30,15	37	Rain this Month 1,187 Inches.	NW	1	Cloudy.
	2 0	42	51	30,12	45			1	Cloudy.
2	8 0	49	51	29,93	49		W	1	Cloudy.
	2 0	45	52	29,93	48		SSW	1	Cloudy, and hazy.
3	8 0	38	48	30,08	36		W	1	Hazy and small rain,
	2 0	46	51	30,02	47		WSW	1	Cloudy and hazy.
4	8 0	40	51	29,93	40		W	1	Cloudy.
	2 0	53	58	29,94	54		NE	1,2	Fine.
5	8 0	48	54	29,90	48		NE	1	Cloudy.
	2 0	52	55	29,85	54			1	Cloudy and hazy.
6	8 0	48	57	29,98	48		E	1	Rain.
	2 0	46	58	30,00	49			1	Rain.
7	8 0	50	56	30,01	46		W	1	Cloudy.
	2 0	55	58	30,14	57			1	Cloudy and hazy.
8	8 0	50	59	30,15	49		W	1	Cloudy.
	2 0	52	59	30,15	54		SW	1	Cloudy and hazy.
9	8 0	49	57	30,17	47		W	1	Cloudy.
	2 0	52	58	30,14	53		NNW	1	Cloudy and hazy.
10	8 0	49	55	30,01	48		SW	1,2	Cloudy.
	2 0	52	56	29,98	53		WNW	1	Cloudy—Rain.
11	8 0	50	55	29,84	49		W	2	Cloudy.
	2 0	53	58	29,88	54		W	1	Cloudy.
12	8 0	50	57	29,60	48		WSW	1	Rain.
	2 0	54	59	29,55	56		WNW	1	Cloudy.
13	8 0	48	59	29,49	47		S	1	Rain.
	2 0	38	57	29,48	54		N	1	Cloudy.
14	8 0	34	47	29,83	34		N	1	Fine.
	2 0	39	55	29,92	40		E	1	Fine.
15	8 0	36	52	29,95	35		E	1	Cloudy.
	2 0	33	55	29,92	39		SE	1	Fine.
16	8 0	30	58	29,70	29		E	1	Cloudy.
	2 0	35	57	29,56	36		SW	1	Cloudy, some snow.

METEOROLOGICAL JOURNAL
for December, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Dec. 17	8	0	36	46	29,60	30	Rain this Month 1,187 Inches.	E	1	Thick fog.
	2	0	40	45	29,75	41		WSW	1	Cloudy.
18	8	0	41	47	30,02	36		E	1	Cloudy.
	2	0	46	50	30,11	47		SSE	1	Cloudy.
19	8	0	46	49	30,20	41		S	1.2	Rain.
	2	0	50	54	30,25	51		W	1	Rain.
20	8	0	41	50	30,34	41		S	1	Cloudy.
	2	0	48	52	30,29	50		WSW	1	Cloudy.
21	8	0	46	53	30,07	41		W	1	Cloudy.
	2	0	50	57	30,11	53		NW	1	Cloudy.
22	8	0	38	53	30,12	38		W	1	Fine.
	2	0	47	55	30,11	50			1	Cloudy.
23	8	0	40	52	29,97	38		N by E	1	Cloudy.
	2	0	44	53	29,92	46		N	1	Cloudy.
24	8	0	36	46	29,86	36		N	1	Cloudy.
	2	0	36	48	29,85	44		NNW	1	Cloudy.
25	8	0	32	45	29,84	32		NNE	1	Cloudy.
	2	0	32	49	29,83	36		SSE	1.2	Cloudy.
26	8	0	30	43	29,74	30		E	1	Cloudy.
	2	0	33	47	29,77	35		ESE	1.2	Cloudy and hazy.
27	8	0	31	43	29,86	30		E	1	Cloudy.
	2	0	33	57	29,88	35		E	1	Cloudy and hazy.
28	8	0	30	43	29,89	30		ESE	1	Fine.
	2	0	31	47	29,92	35		E	1	Cloudy.
29	8	0	27	41	29,95	27		E	1	Cloudy.
	2	0	34	44	29,93	28		ENE	1.2	Cloudy.
30	8	0	27	39	29,91	27		E	1	Cloudy.
	2	0	28	44	29,91	29		E	1	Cloudy.
31	8	0	25	38	29,92	25		N	1	Cloudy.
	2	0	29	39	29,87	30		E	1	Fair.

1820.	Thermometer without.			Thermometer within.			Barometer.*			Six's Thermometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	48	19	33,5	57	31	44,4	30,68	28,84	29,87	53	20	34,1	0,444
February	50	27	39,1	59	40	49,6	30,32	28,38	29,95	52	26	39,1	1,425
March	64	28	43,1	67	43	52,9	30,36	28,93	29,88	66	28	43,8	1,325
April	67	37	52,2	67	53	58,6	30,50	29,23	29,92	69	36	52,2	1,514
May	63	45	55,1	71	50	59,9	30,31	29,17	29,65	70	39	55,5	1,393
June	84	50	59,8	79	55	61,4	30,38	29,54	29,93	85	45	59,4	2,331
July	75	53	62,5	77	60	64,2	30,22	29,35	29,93	80	50	62,5	2,807
August	73	51	62,5	75	59	66,2	30,26	29,48	29,98	77	46	61,0	1,315
September	71	41	57,0	74	53	62,0	30,35	29,41	29,98	73	38	56,9	2,058
October	64	40	49,5	61	49	55,5	30,53	28,71	29,67	65	39	49,8	1,582
November	55	31	43,4	61	46	54,2	30,27	29,33	29,84	56	31	43,9	1,000
December	55	25	41,3	59	38	51,2	30,34	29,49	29,94	57	25	41,9	1,187
Whole year			49,9			56,7			29,88			50,0	18,381

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle.

				° ′ ″
June 1819,	-	-	-	24.14.47 west.
June 1820,	-	-	-	24.11.44 west.
Dip. about	-	-	-	71.6

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXI.

PART II.

LONDON,

PRINTED BY W. BULMER AND W. NICOL, CLEVELAND ROW, ST. JAMES'S;
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MDCCCXXI.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

XIV. *An account of experiments to determine the acceleration of the Pendulum in different latitudes. By Captain* Edward Sabine, *of the Royal Regiment of Artillery, F. R. S. and F. L. S.*

Read March 8, 1821.

The clocks and pendulums used in these experiments are the property of the Royal Society, and were prepared by their direction under the immediate superintendance of Captain Kater; who, in a manuscript account presented to the Society, of the instruments furnished to the Expeditions on Northern discovery, has described them as follows:

"The clocks were made by Shelton, and are the same which accompanied Captain Cook round the world: for each clock a pendulum was cast in one piece of solid brass: this was furnished with a knife edge of hard steel perfectly strait, and finished by drawing the edge longitudinally two or three times on a soft hone, so as to take from its sharpness, and thus preclude any alteration from wear; the back of the knife edge bore firmly against a stout cross piece, and the

heads of the screws securing it, were sunk below the surface, and concealed by brass pins, to prevent their being removed: the knife edge was carefully adjusted, so as to be at right angles to the direction of gravity: a very firm support of brass was screwed to the thick plank which forms the back of the clock case; in this were imbedded two pieces of agate, which were ground into portions of hollow cylinders, finished in their places to receive the knife edge of the pendulum: parallel to the agates a small level was fixed in the direction of the cylinders, by means of which they could be placed truly horizontal: an arc divided into degrees and tenths, but which might be read off by estimation to hundredths, was attached to the back of the clock case at the bottom of the pendulum, to give the arc of vibration.

" Each clock was furnished with a triangular support of wood contrived by Dr. Wollaston, and so firmly arranged, that there appears no reason to apprehend any motion in the point of suspension; and it is sufficiently obvious that no change can take place in the length of the pendulum, but such as may arise from a variation in temperature. For the purpose of distinction, the two clocks and the two pendulums, are marked respectively No. 1 and No. 2."

To this description it may be added, that the clocks were cleaned and oiled by Mr. Arnold, in March 1818, previous to the commencement of the experiments, and that these operations have not been needed subsequently. The oil which was used was prepared by Dr. Wollaston. The preparation consisted in submitting it to a low temperature, when the part which remained fluid was separated by a gentle pressure from that which had become solid; the

oil had undergone this process in the year 1803, and had been laid by since that time.

The experiments, of which the account is now presented to the Society, were made in the course of two voyages of discovery in search of a North-west passage; one in the summer of 1818, and the second in the years 1819 and 1820.

In the first of these voyages, the clock and pendulum marked No. 2, were alone employed, No. 1, being sent at the same time to Spitzbergen with Mr. G. Fisher; but both the clocks being disposable when the second expedition was fitted out, they were both embarked, with a view of obtaining double and corresponding results.

It is designed to detail, in succession, the proceedings at each station, where an opportunity was afforded of landing and setting up the clocks; and to conclude, by recapitulating the number of vibrations made by each pendulum in the different latitudes in which it was tried; and by stating the deductions regarding the figure of the earth, which follow from the accelerations which were thus determined.

FIRST VOYAGE.

Island of Brassa, Shetland.

The ships having anchored in Brassa Sound on the 30th of April 1818, for the purpose of procuring a supply of fresh provision, and their stay, although designed to be very short, being understood to depend in some measure on the wind, the clock was landed, and, by the kindness of William Mouatt, Esq. was set up in a lower room at Gardie House,

on the Island of Brassa. It happened unfortunately, that in the interval between the 30th of April and the 3d of May, on which morning the clocks were re-embarked to proceed on the voyage, the state of the weather was such as to prevent the use of the transit instrument. The rate of the clock was therefore ascertained at this station by comparison with a chronometer, the accuracy of the result being of course dependent on the steadiness with which the chronometer maintained its accustomed rate. No. 1024 of Earnshaw, selected on this occasion, had been received on board on the 15th of April, with an assigned rate of gaining one second daily, founded on a trial of several weeks. The longitude of Brassa is not ascertained with sufficient correctness to determine, by a knowledge of the error of the watch on mean time whilst there, the rate since the 15th; but on the 9th of June, being the first good opportunity of lunar observation which occurred subsequently, the Greenwich time obtained by the mean of several sets of distances taken by different observers, and with different instruments, agreed within four seconds of that shown by 1024, with its rate for the interval of 56 days applied. It may be also stated, as affording an inference that the rate had been particularly maintained during the early part of this interval, that a second chronometer of Mr. Earnshaw's, No. 815, had been sent on board, also on the 15th of April, with a rate gaining 0″,54 daily, determined by a similar trial to No. 1024; on the 1st of May 1024 had gained on 815 since the 15th of April 6,3 seconds, being only 6-tenths of a second less than the difference of their respective rates.

The latitude of Mr. Mouatt's house 60° 09′ 42″ N. was

ascertained by a mean of meridian altitudes observed in November 1818, with a sextant and artificial horizon, by Captain Parry, who on this and on all occasions most kindly studied to render me every assistance in his power, consistent with the due performance of his own duties. The house being close to the sea, the height of the clock above low water mark was ascertained by direct measurement 24 feet.

In the subjoined table is given an account of the going of the clock, as compared with No. 1024; the clock was set up on the afternoon of the 30th of April, but the account was not commenced until time had been allowed for the pendulum to acquire the temperature of the room. The temperatures and arcs of vibration are a mean of frequent observations in the times to which they correspond; and as these were of irregular duration, the corrections due to the several means have been given an influence on the general correction, proportioned to the length of their respective periods.

The correction for the arc is the number of vibrations lost by the pendulum in 24 hours, from its vibrating in a circular, instead of a cycloidal arc; the arcs being less than two degrees, the time of a vibration in a circular arc, whose radius is r: the time of a vibration in a cycloid whose axis is $\frac{1}{2}r$: : $1 + \frac{a}{8r}$: 1, (a being the versed sine of the arc), and the errors arising from the greater length of the vibration in a circular arc being nearly as the square of the arc, when the number of vibrations lost by the pendulum at each station in 24 hours by vibrating in an arc of 1 degree has been ascertained, the corrections due to the arcs in which the pendulum did actually vibrate are obtained by multiplying the square of these arcs by the loss for one degree.

The correction for temperature is the reduction of the mean temperatures to 50 degrees, assumed as a convenient standard at which to compare the observations of the first voyage with each other, it being nearly the mean at which they were made; the correction has been computed from the change in the length of the pendulum due to differences of temperature, the expansion of brass being considered +·00220 inches per foot in 180 degrees of FAHRENHEIT.

The correction for the height above the sea, is the part of a vibration lost in 24 hours by the pendulum in vibrating at such elevation instead of at the level of the sea, the force of gravity increasing inversely as the square of the distance from the centre of the earth. The differences of latitude being considerable between the stations at which the clocks have been set up, and the elevations being small, and differing but little from each other, it has not been deemed necessary to diminish this correction, by taking the geological character of the different stations into consideration; the character is however noted wherever it was not previously well known.

The correction for the buoyancy of the atmosphere has been computed in the manner explained by Captain KATER, in his Account of Experiments for determining the length of the Seconds Pendulum, published in the Philosophical Transactions for 1818; the specific gravity of the pendulum being considered 8,4.

Clock 2 at Brassa. Barom. Mean height 29.745 inches.

1818.	Time shown by 1024 gaining 1 second per diem.	Time shown by The Clock.	The Clock's gain. on 1024.	The Clock's gain. on Time.	The Clock's gain. per diem.	Mean arc.	Mean Temp.	Corrections. Arc.	Corrections. Temp.	Corrections. Elev.	Corrections. Buoy.	Corrected vibrations.
	h. m. s.	h. m. s.	s.	s.	s.	°	°	s.	s.	s.	s.	
Apr. 30	18.46.10,5	18.07.10,5										
						1.8117	53					
May 1	3.21.10,75	2.42.52,75										
						1.798	53,6					
	11.55.10,75	11.17.34										
			222	223,9167	116,8488	1.782	54,26	+ 5,3027	+ 2,0164	+ 0,099	+ 6,24	86530,507.
	22.55.10,35	22.18.27										
						1.785	55,4					
2	9.12.10,25	8.36.16,5										
						1,8	56,96					
	16.45.40,25	16.10.22,25										

Being 86530,507 vibrations in a mean solar day, in *vacuo*, at the level of the sea; the temperature being 50°.

Hare Island.

The expedition having ascended Davis' Strait to the 70th degree of latitude, their progress in a northerly direction was interrupted on the 16th of June, by a temporary accumulation of ice. There being a possibility that the detention might be of some days continuance, the instruments were landed on the morning of the 17th, on the NE side of a small island situated on the west coast of Greenland, in latitude 70° 26′, called in some charts Waygat, and in others Hare Island. Being uninhabited, and consequently affording no accommodation, a marquee was pitched for the reception of the clock; and for its more effectual protection, the marquee was itself inclosed within a large laboratory tent, and a stove placed in

the space between the tent and the marquee for the purpose of regulating the temperature. The weather from the 17th, which was the day of landing, to the 20th, when the instruments were re-embarked, was extremely clear and fine, admitting of transit observations on the 18th, 19th, and 20th; the details of which, with the rate of the clock in the intervals, are given in the annexed Table. The latitude of the tents was ascertained by Captain PARRY, by a mean of meridian altitudes of the sun, observed with a sextant and artificial horizon, 70° 26′ 17″ N.; the elevation above the sea was measured 44 feet; the temperatures and arcs of vibration were observed every second hour, the daily mean being inserted in the Table.

Clock 2 at Hare Island: Barom. Mean height 30,1 inches.

1818.	Observ. Times of ☉'s trans.	Mean time of App. noon.	Clock slow.	Daily gain.	Mean arc.	Mean temp.	Corrections. Arc.	Temp.	Elev.	Buoy.	Corrected vibrations.
	h. m. s.	h. m. s.	m. s.				+	—	+	+	
June 18	23.48.35,58	0.00.35,06	11.59,48								
				153,62	1.783	43,9	5,239	2,664	0,1826	6,448	86562,8256
19	23.51.22	0.00.47,86	9.25,86								
				152,51	1.7825	45,6	5,236	1,925			86562,4516
20	23.54.07.31	0.01.00,66	6.53,35							Mean	86562,6386

Being 86562,6386 vibrations in a mean solar day, in *vacuo*, at the level of the sea; the temperature being 50°.

London.

No other opportunity of pursuing these experiments presented itself during the remainder of the voyage; the ships

having returned to England in the autumn of 1818, the clock was disembarked to have its rate in London ascertained. Mr. BROWNE having kindly permitted the observations for this purpose to be made at his house in Portland-place, the clock was set up in a room adjoining the one in which Captain KATER's Experiments for determining the length of the seconds pendulum had been made; the latitude, as stated by Captain KATER, being 51° 31′ 08″,4 N. and the height above the sea 84,5 feet. The clock was going on the 23d of December, and the account taken up on the 24th at noon, and continued through the ten following days, as shown in the annexed Table; the rate of the chronometer with which the clock was compared was supplied by Mr. BROWNE:

Clock No. 2, London; Barom. mean height 30,31 inches; Chron. No. 112, of ARNOLD losing 0,8s per diem.

1818.	Clock fast of 112.	Daily gain.		Mean arc.	Mean temp.	Corrections.		Daily Vibrations.	Corrections.		Corrected vibrations.
		on 112.	on Time.			Arc.	Temp.		Elev.	Buoy.	
	m. s.	s.	s.	°	°	s. +	s. —		s.	s.	
Dec. 24	0 47,3										
		88,7	87,9	1,664	45	4,559	2,18	86490,279			
25	02 16										
		88,8	88	1,69	45,8	4,703	1,838	86490,865			
26	03 44,8										
		88,9	88,1	1,7	44,5	4,759	2,398	86490,461			
27	05 13,7										
		89	88,2	1,7	45	4,759	2,18	86490,779			
28	06 42,7										
		89,1	88,3	1,7	45	4,759	2,18	86490,879	+0,35	+6,457	86497,4.
29	08 11,8										
		88,7	87,9	1,76	44,5	5,1	2,398	86490,602			
30	09 40,5										
		89,4	88,6	1,82	44	5,454	2,62	86491,434			
31	11 09,9										
1819.		88,5	87,7	1,82	44	5,454	2,62	86490,534			
Jan. 1	12 38,4										
		88,5	87,7	1,83	44	5,514	2,62	86490,594			
2	14 06,9										
		87,6	86,8	1,835	43,5	5,544	2,84	86489,504			
3	16 34,5										

Being 86497,4 vibrations in a mean solar day, in *vacuo*, at the level of the sea; the temperature being 50°.

Results of the first Voyage.

Recapitulation.

Place of observation.	Latitude	Vibrations in a mean solar day.	Acceleration.
London	51.31.08,4N	86497,4	33,107 vibrations between London & Brassa.
Brassa	60.09.42N	86530,507	32,1316 between Brassa and Hare Island.
Hare Island	70.26.17N	86562,6386	and 65,2386 between London and Hare Island

SECOND VOYAGE.

It has been already noticed that both clocks were employed on the second expedition; they were placed in my charge early in 1819, to afford time for the determination of their rates in London previously to their embarkation.

The observations, of which the details are given in the subjoined Tables I. and II, were made, by Mr. Browne's permission, in the room already described in Portland-place; the rate of the chronometer with which the clocks were compared being supplied, as before, by Mr. Browne: the temperatures are corrected to 45°, being the mean to which all the observations of the second voyage are reduced;

TABLE I.

Clock No. 1. in London ; Barom. mean height 30,31 in. ; Chron. No. 112, losing 0,8^s per diem.

1818.	Clock fast of 112.	Daily loss. on 112.	Daily loss. on Time.	Mean arc.	Mean temp.	Corrections. Arc.	Corrections. Temp.	Daily vibrations.	Corrections. Elevation.	Corrections. Buoy.	Corrected vibrations, Temp. 45°.
Dec. 24	1.00.22					+	+				
		18,3	19,1	1,6	46,6	4,21	0,702	86385,812			
25	1.00.03,7										
		18,8	19,6	1,622	47,4	4,327	1,055	86385,782			
26	0.59.44,9										
		18,4	19,2	1,6	48	4,21	1,32	86386,33			
27	0.59.26,5										
		17.7	18,5	1,6	48	4,21	1,32	86387,03			
28	0.59.08,8										
		19	19,8	1,6	49	4,21	1,75	86386,16			
29	0.58.49,8								+0,3496	+6,442	86392,5673
		18,9	19,7	1,56	48,5	4,	1,535	86385,835			
30	0.58.39										
		18,6	19,4	1,51	47,5	3,75	1,1	86385,45			
31	0.58.12,3										
1819.		18	18,5	1,5	47,5	3,7	1,1	86386			
Jan. 1	0.57.54,3										
		19	19,8	1,49	47,5	3,651	1,1	86384,951			
2	0.57.35,3										
		19,3	20,1	1,485	47	3.627	0,88	86384.407			
3	0.57,16										

TABLE II.

Clock No. 2, in London ; Barom. mean height 29,88 inches ; Chron. No. 112, losing 1^s. per diem.

1819.	Clock slow of 112.	Clocks gain. on 112.	Clocks gain. Per diem. on 112.	Clocks gain. Per diem. on Time.	Mean arc.	Mean temp.	Correction Arc.	Correction Temp.	Daily vibrations.	Corrections. Elevation.	Corrections. Buoy.	Corrected vibrations. Temp. 45°.
March	m. s.	s.	s.	s.	°	°	s.	s.		s.	s.	
15 Mid.	13.26						+	+				
		86	86	85	1.69	45	4,703	—	86489,703			
16	12.0											
		84	84	83	1.73	50	4,928	2,18	86490,108			
17	10.36									+0,35	+6,34	86496,997.
		84	84	83	1.73	50	4,928	2,18	86490,108			
18	9.12											
		211,5	84,6	83,6	1.73	50	4,928	2,18	86490,708			
21 Noon	5.40,5											

On the return of this expedition to England in the autumn of the following year, 1820, it was judged proper to repeat these observations, for the purpose of ascertaining whether any injury producing an alteration of rate, had been sustained by either of the clocks or pendulums during the voyage; they were accordingly once more set up in Portland-place, and their going compared with Mr. BROWNE'S excellent clock by CUMMING, the rate of which, losing 0,1^s per diem, was furnished by Mr. BROWNE; the details are comprised in the following Tables III. and IV.

TABLE III.

Clock No. 1, in London; Barom. mean height 29,906. Cumming losing 0,1^s. daily.

1820.	Clock fast of Cumming.	Clock's loss. on Cumming.	Per diem. on Cumming.	Per diem. on Time.	Mean arc.	Mean temp.	Corrections. Arc.	Corrections. Temp	Daily vibrations.	Corrections. Elev.	Corrections. Buoy.	Corrected vibrations. Temp. 45°.
	m. s.	s.	s.	s.	°	°	s. +	s. +		s.	s.	
Dec. 5	18.02,1											
		19	19	19,1	1.38	49,14	3.132	1.811	86385,843			
6	17.43,1											
		18,6	18,6	18,7	1.4	50,1	3.223	2.224	86386,747			
7	17.24,5											
		19,5	19,5	19,6	1.4	50,2	3.223	2.268	86385,891			
8	17.05											
		19,9	19,9	20	1.4	50,5	3.223	2.398	86385,621	+ 3,496	+ 6,335	86392,3353
9	16.45,											
		19,1	19,1	19,2	1.4	50,3	3.223	2.312	86386,335			
10	16.26											
		20,2	20,2	20,3	1.41	51,1	3.27	2.664	86385,634			
11	16.05,8											
		76,8	19,2.	19,3	1.4	47,7	3.223	1.186	86385,101			
15	14.49											

TABLE IV.

Clock No. 2 in London ; Barom. mean height 29,906 inches. Cumming losing 0,1ˢ. daily.

1820.	Clock fast of Cumming.	Clocks Gain. on Cumming.	Clocks Gain. Per diem. on Cumming.	Clocks Gain. Per diem. on Time.	Mean Arc.	Mean temp.	Corrections. Arc.	Corrections. Temp.	Daily vibrations.	Corrections. Elev.	Corrections. Buoy.	Corrected vibrations. Temp. 45°.
	m. s.	s.	s.	s.	°	°	s. +	s. +		s.	s.	
Dec. 5	8.45,2											
		82,1	82,1	82,	1.73	52,8	4,928	3,409	86490,337			
6	10.07,3											
		82,6	82,6	82,5	1.73	53,6	4,928	3,752	86491,18			
7	11.29,9											
		81,9	81,9	81,8	1.73	53,25	4,928	3,6	86490,328			
8	12.51,8											
		81,8	81,8	81,7	1 73	53,2	4,928	3,578	86490,206	+ 0,35	+ 6,3	86496,9741
9	14.13,6											
		82,4	82,4	82,3	1.73	53,3	4,928	3,622	86490,85			
10	15,36											
		81,6	81,6	81,5	1.73	54	4,928	3,92	86490,348			
11	16,57,6											
		330,2	82,55	82,45	1.73	51	4,928	2,62	86489,998			
15	22.27,8											

The very near agreement of the results in Tables III. and IV, with those in Tables I. and II, afforded a satisfactory proof, that no part of the apparatus had suffered by any of the accidents to which instruments are liable on a voyage of such length and peculiar circumstances, and against which the utmost precaution and care may not always be able to provide.

Rate of the clocks in London deduced from the preceding Tables.

	Vibrations in a mean solar day, Temperature 45°.	
Clock 1.	Table I. Dec. 1818. 86392,5673	86392,4513
	Table III. Dec. 1820. 86392,3353	
Clock 2.	Table II. March 1819. 86496,997	86496,9855
	Table IV. Dec. 1820. 86496,9741	

Whilst the observations which have been detailed were making in 1819, it suggested itself, (at first only as a matter of curiosity), to ascertain what difference would take place in the number of vibrations made in a day by each pendulum, on its being removed into the clock numbered differently from itself. Accordingly before the instruments quitted Portland-place to be embarked, No. 1 pendulum was shifted into the clock numbered 2, and No. 2 pendulum into clock No. 1, when the following observations were made; the rate of 112 being supplied, as before, by Mr. BROWNE.

Pendulum 1 in clock 2, London; Bar. mean height 30,2 inches. Chron. No. 112 losing 0,8s. per diem.

1819.	Clock fast of 112.	Daily loss.		Mean arc.	Mean temp.	Corrections.		Daily Vibrations.	Corrections.		Corrected vibrations, Temp. 45°.
		on 112.	on Time.			Arc.	Temp.		Elevation.	Buoy.	
Jan.	m. s.	s.	s.	°	°	s.	s.			s.	
3 Mid.	6.02,5					+	+				
		23,5	24,3	1.74	46,25	4,979	0,55	86381,229			
4	5.39								+	+	
		24	24,8	1.775	47,25	5,181	0,99	86381,371	0,3496	6,43	86388,0967
5	5.15										
		24,1	24,9	1.77	47,5	5,152	1,1	86381,352			
6	4.50,9										

Pendulum 2 in clock 1, London; Bar. mean height 30,2 inches. Chron. No. 112 losing 0,8s. per diem.

1819.	Clock fast of 112.	Daily gain.		Mean arc.	Mean temp.	Corrections.		Daily Vibrations.	Corrections.		Corrected vibrations. Temp. 45°.
		on 112.	on Time.			Arc.	Temp.		Elevation.	Buoy.	
Jan.	h. m. s.	s.	s.	°	°	s.	s.		s.	s.	
3 Mid.	0.58.57,5					+	+				
		136,4	135,6	0,913	49	1,373	1,75	86538,723			
4	1.01.13,9								+	+	
		134,6	133,8	0,983	50	1,592	2,18	86537,572	0,3503	6,398	86545,0623
5	1.03.28,5										
		135,4	134,6	1	50,5	1,647	2,4	86538,647			
6	1.05.43,9										

Whence it appeared that the pendulum No. 1, vibrating in clock No. 2, would make 86388,0967 vibrations; and the pendulum No. 2, vibrating in clock No. 1, 86545,0623 vibrations, in a mean solar day, in *vacuo*, at the level of the sea, the temperature being 45°.

Melville Island.

Only one opportunity presented itself in the course of the second voyage of setting up the Pendulum clocks; this was during the detention of the ships at Melville Island, in the Polar Sea, in the winter of 1819—1820.

The time afforded for the observations at this station was limited by the nature of the climate alone; they were accordingly continued until the rates of the clocks were obtained with much accuracy: it has been thought proper, therefore, to give a more circumstantial detail of the proceedings at this station, than at those of the former voyage.

As soon as the harbour was determined in which it was purposed to secure the ships for the winter, and whilst a canal was cutting to admit them through the ice by which it was already occupied, its shores were carefully examined, with a view to select a suitable spot for an observatory.

The land was found of little elevation, and generally level, except where intersected by ravines, being the courses in which the winter's fall of snow drained on dissolution to the sea. The soil, which appeared by the banks of these channels to be many feet in depth, consisted of sand intermixed with small stones, being the debris of the sandstone rock of which the island is composed; it was at this time consolidated by the frost, and was harder than the original rock, but much the greater part bore evident marks of being swampy at times; and even the more elevated spots afforded little prospect of a solid foundation for the clock-stands on the return of summer.

However, as no preferable situation could be found within

such distance from the ships, as it would have been convenient, or indeed prudent, to venture, one of these was fixed on; and it was hoped that by sinking the legs of the stands a few inches into the frozen soil, and by commencing the experiments as early in the ensuing year as the season should admit, they might be completed before the ground should be affected by a thaw.

It was desirable therefore to be thoroughly prepared before the severity of the winter should set in; accordingly when the ships had been secured, and a party of men could be spared for the occasion, an observatory house was commenced. The house was built of the store plank and boards carried by the ships, care being taken to cut or injure them as little as possible; the walls were weather-boarded, lined, and filled in between with moss; the roof was protected by a tarpaulin covering: it was divided into two rooms, whereof the inner, being designed for the reception of the clocks, was warmed by pipes proceeding from a stove placed in the outer room; the floors were boarded, and the walls furnished on the inside with Russia matting. The house was finished and the clocks moved into it before the end of October.

If any hope had been entertained of being able to do more during the winter than merely to prepare for the return of more favourable weather, it was ended by the severity of cold, far exceeding expectation, with which November set in. From this date until the close of March, the highest degree registered by a thermometer, suspended in the air, was $+ 6^\circ$ of FAHRENHEIT, and in no one of these five months did the mean temperature rise above -18°; under such circumstances, an attempt to raise the temperature of the house,

sufficiently to carry on the experiments, and to keep it up during their course, with the requisite steadiness and uniformity, must have altogether failed. It may not be amiss to remark, that notwithstanding the house was as effectual for the purpose as the utmost liberality in the supply of materials, with no labour spared in their application, could produce, a very little wind with so low a temperature abstracted the heat with such rapidity, that the influence of the stove was scarcely felt beyond its immediate vicinity; and a thermometer placed in those parts of the inner room where the clocks would have stood, could not be kept above zero, with such fire in the stove as it would have been prudent to maintain.

The clocks were therefore suffered to remain unpacked during the winter in the inner room, whilst the outer served a variety of useful purposes, which could not have been conveniently effected on board ship.

On the 24th of February, the matting with which the walls of the outer room were covered accidentally caught fire, and notwithstanding the endeavours of the persons who were present, the fire was communicated rapidly to the roof; it was at length fortunately extinguished by the exertions of the officers and men from the ships, before the clocks or any part of their apparatus had received injury; the packing chest alone of one was slightly scorched: the only personal sufferer on the occasion was an artilleryman, who had accompanied me on the voyage, and who, in his anxiety to place the instruments out of danger, exposed his hands incautiously, and was in consequence so severely frost-bitten, as to render necessary the amputation of three fingers of the left hand, and two of the right.

The house was speedily repaired, the outer room being reduced in size to a porch sufficient to contain the stove; and the inner room, which had scarcely been touched by the fire, remaining as before.

Towards the end of April the sun had influence to keep the thermometer a few degrees above zero for some hours of the day. The clocks were now unpacked and set up: the flooring being removed, the legs of the stands were placed on sleepers sunk some inches into the frozen ground in grooves which were excavated by crow bars.

It may be worthy of remark, that when the boxes containing the thermometers which accompany the clocks were opened, the mercury was observed to be retired into the bulbs and frozen, although the temperature of the air had not been so low as the freezing point of mercury for several weeks. The thermometer boxes were enclosed each with the pendulum to which it belonged, in a stout case of oak; and these again were contained in chests holding each one clock with its apparatus complete. The thermometers had been thoroughly cooled in their cases by the long continued severity of the winter; but the warmth had not yet made its way through such a multiplicity of enclosures. It may be also mentioned, in proof of the slowness with which such a mass of solid brass as constituted the bob of the pendulums conforms to the temperature of the surrounding atmosphere, compared with the mercury in the thermometer tubes, that several hours had elapsed, after the pendulums were taken out of their cases, (when it is presumed they also may have been at —40°) before they ceased to cause a deposit of moisture from the air of the room, which was about the same number

of degrees above zero : the mercury in the thermometers, on the other hand, took up the temperature of the room within half an hour after their exposure. The clocks were put in motion on the 30th of April, and the account taken up on the 4th of May, the room having been kept at about the temperature of $+45°$ for the preceding three days and nights.

It proved however an erroneous supposition, that a sufficient interval to complete the observations would occur between the first return of mild weather, and the thorough breaking up of the frost: it was not indeed until the third week in May, that the weather became sufficiently settled or warm, to commence them to any purpose : during the first fortnight strong northerly winds prevailed with heavy snow drifts, preventing the reference to a meridian mark, and occasionally burying the house altogether beneath the drift snow ; when the only access to it was by digging down to the window of the room in which the clocks were going. It was desirable to keep the temperature of the room at about the same degree as in the experiments in London, namely, about $45°$; but the mean height of the thermometer in the air during this fortnight was not more than $+6°$. The walls of the house had been hung round with a double folding of canvass, and were well banked up with snow on the outside; nevertheless, it was necessary to introduce the stove into the same room with the clocks to effect so great a difference of temperature, and even to place it not far distant from them. It will readily be imagined that a forced temperature of such amount, and under such circumstances, must have been liable to incessant fluctuation and uncertainty, as indeed it was. The thermometers were suspended in the clock cases in such manner that their bulbs were on a level

with the bobs of the pendulums, and as near them as the extent of their arcs of vibration would admit: it is probable, therefore, that each thermometer was an index to the variations in temperature to which the principal part of its pendulum was subjected; but other thermometers, placed one or two feet higher in the clock cases, so far from corresponding, frequently differed many degrees from the lower one. Every gradation between the temperature of the external air and that of the stove, might be remarked at the same time by thermometers placed in different parts of the room; nor was it possible to provide against effects which changed with the situation of the sun and the direction of the wind. The thermometer was registered frequently in the hour, but the result was necessarily very unsatisfactory. Moreover, in consequence of the introduction of the stove into the room, the ground beneath the nearest legs of the stand were softened, and the levels affected, which might not have been the case could the room have been sufficiently heated by pipes.

The mean height of the thermometer in the air during the second fortnight in May was between 24° and 25°; it is probable that the registry of the temperature of the clocks became then an approximation to the truth; but this is by no means certain, as there was yet very much inequality between the heat of the days and of the nights, the latter being still very cold; but the effects of the heat acquired by the land were now becoming manifest every where, and advancing with great rapidity.

It was soon found impracticable to keep the levels in any thing like adjustment; and by the end of the fortnight, the thaw prevailed to such an extent as to oblige the aban-

donment of the house, before any satisfactory conclusions had been obtained.

Towards the middle of June, the land was tolerably clear of snow in the neighbourhood of the harbour; the mean temperature of the air had become but little less than that at which it was desired to carry on the experiments; and the range of the thermometers in the course of the twenty-four hours had greatly diminished. An elevated and dry spot was now chosen, and the earth being removed for nearly two feet in depth (it having thawed above one foot), a foundation for the stands was made with as large stones as could be brought for the purpose, filled in with sand; the clocks were then set up, and protected by a marquee, pitched, as at Hare Island, within a laboratory tent, a stove being placed at the door of the marquee with pipes leading through the tent.

The clocks were going on the 18th of June; but a heavy gale of wind continuing through the two following days, forced the pegs and other fastenings of the tent on the weather side (it being bad holding ground), and bore it down on the marquee, until relief was sent from the Hecla, when the tent was permanently secured by ice anchors. Whilst the marquee sustained the weight of the tent, the inner walls were unavoidably pressed in several places against the clocks and stands, which were shaken thereby; one of the clocks was also stopped for the purpose of putting its pendulum in safety. This difficulty being passed, no other interruption took place to the success of the experiments. The foundation subsided a little at first, but soon became sufficiently firm and steady; a fire was generally required in the stove at night, but only occasionally during the day, the tem-

perature being very regular and satisfactory; a thermometer suspended in the marquee on a level with the dial plates of the clocks, rarely differing more than one or two degrees at farthest from those within the cases.

In the following tables, [A. B. C.] an account is given of the going of the clocks from the 20th of June to the 14th of July, when it was conceived that a sufficient number of results had been obtained, exclusive of those before the foundation had settled, which are omitted.

The times of transit were noted by a very steady going chronometer, No. 259 of Messrs. PARKINSON and FRODSHAM. The distance being small between the tent and the observatory, the chronometer could be carried from one to the other, without inconvenience, as often as was required, by which means the comparison of intermediate watches was avoided.

The mark to which the transit instrument was adjusted previously to every observation, was about three hundred yards distant, being as far as could be distinctly seen at all times; other marks were fixed in the prolongation of the same line at distances of one and a half, and three miles, by which the position of the first was occasionally verified.

The clocks were compared with No. 259,

1st. At every revolution of twelve hours by the chronometer, the daily rate of which was less than two seconds; the time of comparison was, whenever No. 259 showed seven hours, or more precisely of clock 1, one minute before, and of clock 2, one minute after seven hours; the sun's transit hav-

Table C, *to face p.* 185.

By Transits of Arcturus, South of the Zenith; Barom. mean height 29,864 inches.

Chronometer and Clock, No. 1.

1820.	Observ. Times of Transits.	Difference.	Inter-val.	Diff. between mean solar and Sid. days.	259's Gain.	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean arc.	Mean temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day. Temp. 45 degrees.	
	h. m. s.	m. s.	days.	m. s.	s.	m. s.	s.	s.	s.	s.	°	°	s.	s.		
June 26	14.22.04,36					20.11										
		7.50,26	2	7.51,82	1,56		111,	112,56	56,28	56,434	1.4625	44,5	+ 3,521	—0,218	86459,737	
28	14.14.14,1					22.03										
		19.32,73	5	19.39,55	6,82		268,6	275,42	55,084	55,235	1.512	46,29	3,763	+0,567	86459,565	
July 3	13.54.41,37					26.31,6										
		7.48,67	2	7.51,82	3,15		108,4	111,55	55,775	55,928	1.503	45,443	3,718	+0,193	86459,839	
5	13.46.52,7					28.20										
		3.54,61	1	3.55,91	1,3		54,5	55,8	55,8	55,953	1.533	46,664	3,868	+0,73	86460,551	86459,896 or corrected for the buoyancy of the atmosphere. 86466,272.
6	13.42.58,09					29.14,5										
		7.48,99	2	7,51,82	2,83		107,9	110,73	55,365	55,517	1.515	46,83	3,778	+0,802	86460,097	
8	13.35.09,					31.02,4										
		15.38,03	4	15.43,64	5,61		219,1	224,71	56,178	56,332	1.463	45,1	3,523	+0,044	86459,899	
12	13.19.31,07					34.41,5										
		7.47,64	2	7.51,82	4,18		106,	110,18	55,09	55,241	1.512	48,18	3,763	+1,399	86460,403	
14	13.11.43,43					36.27,5										

Clock, No. 2.

1820.	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean arc.	Mean Temp.	Corrections for arc.	Corrections for Temp.	Vibrations in a mean solar day. Temp. 45 degrees.	
	m. s.	s.	s.	s.	s.	°	°	s.	s.		
June 26	58.51										
		318,8	320,36	160,18	160,62	1.676	44,7	+ 4,63	—0,132	86565,118	
28	64.09,8										
		789,7	796,52	159,304	159,738	1.678	46,49	4,64	+0,654	86565,032	
July 3	77.19,5										
		316,3	319,45	159,725	160,162	1.658	45,79	4,53	+0,345	86565,037	
5	82.35,8										
		158,7	160,	160,	160,438	1.67	47,244	4,596	+0,987	86566,021	86565,268 or corrected for the buoyancy of the atmosphere. 86571,651.
6	85.14,5										
		315,5	318,33	159,165	159,601	1.67	47,38	4,596	+1,047	86564,244	
8	90.30										
		634,5	640,11	160,062	160,5	1.68	45,77	4,651	+0,337	86565,488	
12	101.04,5										
		313,5	317,68	158,84	159,275	1.674	48,72	4,618	+1,635	86565,528	
14	106.18										

By Transits of α Lyræ, South of the Zenith; Barom. mean height 29,90 inches.

Chronometer and Clock, No. 1.

1820.	Observ. Times of Transits.	Difference.	Inter-val.	Diff. between mean solar and Sid. days.	259's Gain.	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean arc.	Mean temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day. Temp. 45 degrees.	
July 5	18.09.32,93					28.30										
		11.42,43	3	11.47,73	5,3		161,5	166,8	55,6	55,753	1.52	46,74	+ 3,803	+0,763	86460,319	
8	17.57.50,5					31.11,5										
		3.54,72	1	3.55,91	1,19		55,6	56,79	56,79	56,946	1.48	44,66	3,606	—0,149	86460,403	86460,032 or corrected for buoyancy 86466,462.
9	17.53.55,78					32.07,1										
		15.38,73	4	15.43,64	4,91		217,9	222,81	55,703	55,856	1.459	45,35	3,505	+0,154	86459,515	
13	17.38.17,05					35.45										
		3.54,02	1	3.55,91	1,89		52,2	54,09	54,09	54,243	1.56	51,05	4,006	+2,642	86460,89	
14	17.34.23,38					36.37,2										

Clock, No. 2.

1820.	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean arc.	Mean Temp.	Corrections for arc.	Corrections for Temp.	Vibrations in a mean solar day. Temp. 45 degrees.	
July 5	83.05										
		473,5	478,8	159,6	160,037	1.67	47,26	+ 4,596	+ 0,994	86565,627	
8	90.58,5										
		159	160,19	160,19	160,63	1.67	45,283	4,596	0,126	86565,352	86565,344 or corrected for buoyancy, 86571,783.
9	93.37,5										
		633,5	638,41	159,602	160,039	1.674	45,97	4,618	0,422	86565,079	
13	104.11										
		155,7	157,59	157,59	158,022	1.681	51,56	4,657	2,864	86565,543	
14	106.46,7										

By Transits of α Aquilæ, South of the Zenith; Barom. mean height 29,935 inches.

Chronometer and Clock, No. 1.

1820.	Observ. Times of Transits.	Difference.	Inter-val.	Diff. between mean solar and Sid. days.	259's Gain.	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean arc.	Mean temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day. Temp. 45 degrees.	
July 5	19 20.31,17					28.32,2										
		11.42,52	3	11.47,73	5,21		162,3	167,51	55,837	55,99	1.515	46,67	+ 3,778	+ 0,733	86460,501	86460,177 or corrected 86466,568
8	19.08.48,65					31.14,5										
		7.49,8	2	7.51,82	2,02		109,6	111,62	55,81	55,963	1.487	45,2	3,64	0,088	86459,691	
10	19.00.58,85					33.04,1										

Clock, No. 2.

1820.	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean arc.	Mean Temp.	Corrections for arc.	Corrections for Temp.	Vibrations in a mean solar day. Temp. 45 degrees.	
July 5	83.12,8										
		473,8	479,1	159,7	160,137	1.67	47,26	+ 4,596	+ 0,994	86565,727	86565,555 or corrected 86571,953
8	91.06,6										
		317,9	319,92	159,96	160,397	1.672	45,68	4,607	0,297	86565,301	
10	96.24,5										

By Transits of the Sun, North of the Zenith; Barom. mean height 29,864 inches.

Table B, *to face p.* 185.

Chronometer.

1820	Observ. Times of Transits.	Mean Time of App. Midnight.	259 fast of mean time.	Interval.	259's gain.
	h. m .	h. m. s.	h. m. s.	days.	s.
June 25	18.37.29,49	12.02.21,49	6.35.08		
				3	2,4
28	18.38.08,57	12.02.58,17	6.35.10,4		
				6	8,62
July 4	18.39.25	12.04.05,98	6.35.19,2		
				1	1,72
5	18.39.37,08	12.04.16,34	6.35.20,74		
				4	6,42
9	18.40.21,38	12.04.54,22	6.35.27,16		
				4	6,33
13	18.40.59,18	12.05.25,29	6.35.33,49		
				1	2,07
14	18.41.07,55	12.05.31,99	6.35.35,56		

Clock No. 1.

1820	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem.	Mean Arc.	Mean temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day, Temp. 45 degrees.	
	m. s.	s.	s.	s.	°	°	s. +	s. +		
June 25	19.27,4									
		165,6	168,	56,	1,482	45,24	3,615	0,1	86459,715	86459,96, or corrected for buoyancy 86466,336.
28	22.13									
		325	333,62	55,603	1,507	45,84	3,738	0,366	86459,707	
July 4	27.38									
		53,2	54,92	54,92	1,533	47,41	3,868	1,06	86459,848	
5	28.31,2									
		217,8	224.22	56,055	1,506	46,14	3,733	0,486	86460,274	
9	32.09									
		218,7	225,03	56,258	1,46	45,42	3,509	0,184	86459,951	
13	35.47,7									
		52,3	54,37	54,37	1,56	51,23	4,006	2,721	86461,097	
14	36.40									

Clock No. 2.

1820	Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day, Temp. 45 degrees.	
	m. s.	s.	s.	s.	°	°	s. +	s. +		
June 25	56.41,8									
		477,9	480,3	160,1	1.674	45,25	4,618	0,11	86564,828	86565,294, or corrected for buoyancy 86571,677.
28	64.39,7									
		951,8	960,42	160,07	1.672	46,11	4,607	0,488	86565,165	
July 4	80.31,5									
		158	159,72	159,72	1.665	47,73	4,569	1,199	86565,488	
5	83.09,5									
		634	640,42	160,105	1.67	46,72	4,596	0,755	86565,456	
9	93.43,5									
		636	642,33	160,583	1,674	46,03	4,618	0,453	86565,654	
13	104.19,5									
		155,5	157,57	157,57	1.681	51,77	4,657	2,956	86565,183	
14	106.55									

By Transits of Capella, North of the Zenith; Barom. mean height 29,864 inches.

Chronometer.

	Observ. Times of Transits.	Difference.	Interval.	Differ. between m. solar & sid. days.	259's gain.
	h. m. s.	m. s.	days.	m. s.	s.
June 28	17.09.38,37				
		27.20,07	7	27.31,37	11,3
July 5	16.42.18,3				
		11.42,58	3	11.47,73	5,15
8	16.30.35,72				
		23.27,14	6	23.35,46	8,32
14	16.07.08,58				

Clock No. 1.

	Fast of 259.	Gain on 259.	Gain on sid. time.	Gain per diem Sidereal	Gain per diem Solar.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day, Temp. 45 degrees.	
	m. s.	s.	s.	s.	s.	°	°	s. +	s. +		
June 28	22.09,4										
		377,1	388,4	55,486	55,638	1.51	46,074	3.753	0,472	86459,863	86459,97, or cor. for buoyancy 86466,346.
July 5	28.26,5										
		162	167,15	55,717	55,87	1.52	46,8	3.803	0,789	86460,462	
8	31.08,5										
		325,2	333,52	55,587	55,738	1.48	46,15	3.606	0,506	86459,85	
14	36.33,7										

Clock No. 2.

	Fast of 259.	Gain on 259.	Gain on sid. time.	Gain per diem Sidereal,	Gain per diem Solar.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean solar day, Temp. 45 degrees.	
	m. s.	s.	s.	s.	s.	°	°	s. +	s. +		
June 28	64.29										
		1106,5	1117,8	159,686	160,123	1,67	46,34	4,596	0,589	86565,308	86565,338, or cor. for buoyancy 86571,721.
July 5	82.55,5										
		473,5	478,65	159,55	159,987	1,67	47,32	4,596	1,02	86565,603	
8	90.49										
		948,2	956,52	159,42	159,857	1,674	46,75	4,618	0,77	86565,245	
14	106.37,2										

Table A, *to face p.* 185.

By Transits of the Sun, South of the Zenith; Barom. mean height 29,864 inches.

1820	Chronometer.					Clock No. 1.								
	Obsd. Times of Transits.	Mean Time of Appar. Noon.	259 Fast of Mean Time.	Inter-val.	259's Gain.	Fast of 259.	Gain. on 259.	Gain. on Time.	Gain. per diem.	Mean Arc.	Mean Tempe-rature.	Corrections. for Arc.	Corrections. for Tem-perature.	Vibrations in a mean Solar day, Temp. 45°.
	h. m. s.	h. m. s.	h. m. s.	days	s.	m. s.	s.	s.	s.	°	°	s. +	s.	
June 25	6.37.22,81	0.02.15,25	6.35.07,56			19.00,5								
				1	0,78		54,5	55,28	55,28	1.496	46,16	3,684	+0,51	86459,474
26	6.37.36,05	0.02.27,71	6.35.08,34			19 55								
				4	4,64		218,2	222,84	55,71	1.497	45,59	3,689	+0,257	86459,656
30	6.38.28,88	0.03.15,9	6.35.12,98			23.33,2								
				2	2,31		108,8	111,11	55,555	1.5	46,16	3,704	+0,51	86459,769
July 2	6.38.54,13	0.03.38,84	6.35.15,29			25.22								
				2	3,03		108,5	111,53	55,765	1.493	45,69	3,669	+0,301	86459,735
4	6.39.19,11	0.04.00,79	6.35.18,32			27.10,5								
				1	2,01		54,2	56,21	56,21	1.505	45,34	3,728	+0,15	86460,088
5	6.39.31,63	0.04.11,3	6.35.20,33			28.04,7								
				1	1,83		54,4	56,23	56,23	1.533	47,09	3,868	+0,919	86461,017
6	6.39.43,64	0.04.21,48	6.35.22,16			28.59,1								
				1	1,44		54,4	55,84	55,84	1.497	45,336	3,689	+0,148	86459,677
7	6.39,54,88	0.04.31,28	6.35.23,6			29.53,5								
				1	1,26		54,1	55,36	55,36	1.522	47,044	3,813	+0,899	86460,072
8	6.40.05,67	0.04.40,81	6.35.24,86			30.47,6								
				2	2,48		108,3	110,78	55,39	1.5	46,31	3,704	+0,576	86459,67
10	6.40.25,95	0.04.58,61	6.35.27,34			32.35,9								
				1	1,58		55,3	56,88	56,88	1.43	44,57	3,366	—0,188	86460,058
11	6.40.35,78	0.05.06,86	6.35.28,92			33.31,2								
				3	5,26		162,8	168,06	56,02	1.484	46,33	3,625	+0,585	86460,23
14	6.41.02,91	0.05.28,73	6.35.34,18			36.14								

86459,897, or corrected for buoyancy 86466,273.

1820	Clock No. 2.								
	Fast of 259.	Gain. on 259.	Gain. on Time.	Gain. per diem.	Mean Arc.	Mean Tempe-rature.	Corrections. for Arc.	Corrections. for Tem-perature.	Vibrations in a mean solar day, Temp. 45°.
	m. s.	s.	s.	s.	°	°	s. +	s.	
June 25	58.01,2								
		635,5	640,14	160,035	1.68	45,76	4,651	+0,332	86565,018
26	68.36,7								
		317,8	320,11	160,055	1.6735	46,37	4,615	+0,603	86565,273
30	73.54,5								
		317	320,03	160,015	1.662	45,94	4,552	+0,409	86564,976
July 2	79.11,5								
		158,7	160,71	160,71	1.66	45,76	4,541	+0,332	86565,583
4	81.50,2								
		159	160,83	160,83	1.666	47,7	4,574	+1,186	86566,59
5	84.29,2								
		158,8	160,24	160,24	1.672	45,81	4,607	+0,353	86565,2
6	87.08								
		158	159,26	159,26	1.666	47,584	4,574	+1,135	86564,969
7	89.46								
		317	319,48	159,74	1.672	47	4,607	+0,88	86565,227
8	95.03								
		159,4	160,98	160,98	1.673	45,2	4,613	+0,088	86565,681
10	97.42,4								
		474,6	479,86	159,953	1.674	47	4,618	+0,88	86565,451
11	105.37								

86565,3, or corrected for buoyancy 86571,683.

ing preceded the comparison within half an hour. The rate of 259 on mean time from one noon transit to the next, and from one midnight transit to the next, being added (as the rate was always gaining) to the gain of each clock on 259 in the corresponding interval, showed the gain of the clocks respectively on mean time, as deduced from transits of the sun.

2nd. Clock 1 was compared at nine minutes, and clock 2 at eleven minutes, after a star had been observed to pass the middle wire of the transit; the gain of 259 between the transits of the star being applied, as before, to the gain of the clocks on the chronometer, their rates were obtained in the intervals of sidereal time.

The temperatures were registered every hour, and the arcs of vibration every third hour: the temperatures were occasionally noted by the serjeant of artillery, the arcs always by myself.

The results in one view of the Tables A. B. and C. are as follow;

By Transits of	Vibrations per diem.	
	Clock 1.	Clock 2.
The Sun, S. of the Zen. mean of 18 days	86466,273	86571,683
The Sun, N. of the Zen. mean of 19 days	86466,336	86571,677
Capella, N. of the Zen. mean of 16 days	86466,346	86571,721
α Lyræ, S. of the Zen. mean of 9 days	86466,462	86571,783
α Aquilæ, S. of the Zen. mean of 5 days	86466,568	86571,953
Arcturus, S. of the Zen. mean of 18 days	86466,272	86571,651

Each result being given a value proportioned to the number of days of which it is the average, the means are obtained of clock No. 1, 86466,338 vibrations, and of clock No 2, 86571,7165 vibrations per diem.

The elevation of the clocks above the sea was ascertained

by a theodolite, the telescope of which being placed on a level with the pendulums, and adjusted horizontally, the height on the ship's mast cut by the middle wire was carefully noted: this height was thirty-four feet six inches above the low water mark on a graduated tide pole, which was moored to the bottom near the ship. The measurement was repeated with the magnetic transit instrument, and gave precisely the same result. The distance between the station and the ship being about seven hundred yards, the true height may be considered thirty-four feet, the six inches being omitted in compensation of the distance. The corrections due to this elevation are for clock 1, +0,1413, and for clock 2, +0,1415, making the final results 86466,4793 and 86571,858 vibrations in a mean solar day, in *vacuo*, at the level of the sea; the temperature being 45°.

It has been noticed, that previous to the embarkation of the clocks on the voyage, they had gone for a few days in London with the pendulums interchanged, with a view to ascertain the comparative influence of the maintaining power of the clocks, on the number of vibrations made by each pendulum.

The preceding observations having been concluded at Melville Island on the 14th of July, at which time there appeared no immediate prospect of putting to sea as no water was yet visible, a second series was commenced with the pendulums placed in the clocks marked differently from themselves; the ships did not quit the harbour until the 1st of August, which afforded sufficient time for the completion of this series also; and thus four results were obtained, instead of two, towards the deduction of the acceleration of the pendulum at Melville Island.

By Transits of the Sun, South of the Zenith.

Table A, *to face p.* 186.

1820.	Chronometer.					Pendulum 2, in Clock 1. Barom. mean height, 29,653 inches.									Pendulum 1, in Clock 2. Barom. mean height, 29,76 inches.								
	Observ. Times of Transit.	Mean Time of App. Noon.	259 fast of mean time.	Inter-val.	259's gain.	Fast of 259.	Gain.			Mean Arc.	Mean Temp.	Corrections.		Vibrations in a mean Solar day, Temp. 45°	Fast of 259.	Gain			Mean Arc.	Mean Temp.	Corrections.		Vibrations in a mean Solar day, Temp. 45°
							on 259.	on Time.	per diem.			for Arc.	for Temperature.			on 259.	on Time.	per diem.			for Arc.	for Temp.	
	h. m. s.	h. m. s.	h. m. s.	days.	s.	m. s.	s.	s.	s.	°	°	s. +	s.		m. s.	s.	s.	s.	°	°	s. +	s.	
July 17	6.41.22,92	0.05.46,17	6.35.36,75			10.20,3									4.52,3								
				1	1,89		208,7	210,59	210,59	0,92	50,025	1.396	+2,189	86614,175		47,9	49,79	49,79	1,71	49,59	4,813	+2,007	86456,61
18	6.41.29,55	0.05.50,91	6.35.38,64			13.49									5.40,2								
				1	0,61		211,7	213,31	213,31	0,86	45,1	1.22	+0,04	86613,57		50,1	50,71	50,71	1,698	44,48	4,746	—0,22	86455,236
19	6.41.34,38	0.05.55,13	6.35.39.25			17.20,7									6.30,3								
							212,2	212,99	212,99	0,894	44,97	1.318	—0,013	86614,295		50,5	51,29	51,29	1,71	43,	4,813	—0,88	86455,223
20	—	—	—	2	1,59	20.52,9									7.20,8								
							212,1	212,9	212,9	0,9	44,9	1.336	—0,04	86614,196		50,5	51,3	51,3	1,669	44,62	4,585	—0,167	86455,718
21	6.41.42,6	0.06.01, 6	6.35.40,84			24.25									8.11,3								
				1	1,21		210,5	211,71	211,71	0,939	46,9	1.454	+0,832	86613,996		52,2	53,41	53,41	1,332	47.	2,92	+0,88	86457,21
22	6.41.46,22	0.06.04,17	6.35.42,05			27.55,5									9.03,5								
							211,	212,12	212,12	0,899	44,63	1.333	—0,162	86613,291		52,5	53,62	53,62	1,56	43,86	4,006	—0,5	86457,126
23	—	—	—	2	2,2	31.26,5									9.56.								
							211,5	212,63	212,63	0,872	44,44	1.254	—0,244	86613,64		51,7	52,83	52,83	1,564	44,03	4,026	—0,422	86456,434
24	6.41.51,64	0.06.07,34	6.35.44,3			34.58									10.47,7								
				1	1,34		210,9	212,24	212,24	0,95	47,63	1.488	+1,155	86614,883									
25	6.41.53,59	0.06.07,95	6.35.45,64			38.28,9																	
							210	210,97	210,97	0,91	48,21	1.366	+1,41	86613,746									
26	—	—	—			41.58,9																	
				3	2,92		211,6	212,57	212,57	0,82	44,7	1.109	—0,13	86613,549									
27	—	—	—			45.30,5																	
							210,8	211,78	211,78	0,88	44,	1.277	—0,44	86612,617									
28	6.41.54,96	0.06.06,4	6.35.48,56			49.01,3																	
				1	2,04		210,2	212,24	212,24	0,9	45,15	1.336	+0,066	86613,642									
29	6.41.55,35	0.06.04.75	6.35.50,6			52.31,5																	
				1	1,98		213	214,98	214,98	0,79	43,54	1.029	—0,64	86615,368									
30	6.41.55	0.06.02,42	6.35.52,58			56.04,5																	
				1	1,74		211,3	213,04	213,04	0,915	44,9	1.381	—0,04	86614,381									
31	6.41.54	0.05.59,68	6.35.54,32			59.35,8																	

86613,953, or corrected for the buoyancy of the atmosphere, 86620,293.

86456,222, or corrected for buoyancy, 86462,583.

By Transits of the Sun, North of the Zenith. Barom. mean height 29,76 inches.

1820.	Chronometer. Observ. Times of Transit.	Mean Time of App. Midnight.	259 fast of Mean Time.	Inter-val.	259's Gain.
	h. m. s.	h. m. s.	h. m. s.	days.	s.
July 17	18.41.27,36	12.05.48,64	6.35,38,72		
				7	7
24	18.41.53,44	12.06.07,72	6.35.45,75		

1820.	Pendulum 1, in Clock 2. Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean Solar day, Temp. 45°.
	m. s	s.	s.	s.	°	°	s.	s.	
July 17	5.15,7								
		354,8	361,8	51.686	1.592	45.04	+4,172	+0,017	86455,875, or cor. for buoyancy 86462,236.
24	11.10,5								

1820.	Pendulum 2, in Clock 1. Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean Solar day, Temp. 45°.
	m. s.	s.	s.	s.	°	°	s.	s.	
July 17	12.00,5								
		1482,2	1489,2	212,743	0,895	45,4	+1.321	+0,175	86614,239, or cor. for buoyancy, 86620,612.
24	36.42,7								

By Transits of Stars South of the Zenith. Barom. mean height, 29,76 Inches.

820.	Chronometer. Observ. Times of Transit.	Difference.	Inter-val.	Diff. between Solar & Sidereal days.	259's Gain.
	Arcturus.				
ly 17	12.59.58	m. s.	days.	m. s.	s.
		3.54,55	1	3.55,91	1,36
18	12.56.03,45				
		23.28,95	6	23.35,46	6,51
24	12.32.34,5				
		3.55,08	1	3.55,91	0,83
25	12.28.38,92				
	α. Lyræ.				
17	17.22.39,02				
		3.54,37	1	3.55,91	1,54
18	17.18.44,65				
		23,30,2	6	23.35,46	5,26
24	16.45.14,48				
		7.49,6	2	7.51,81	2,21
26	16.47.24,85				
	α. Aquilæ.				
17	18.33.37,08				
		3.54.05	1	3.55,91	1,86
18	18.29.43,03				
		23.29,83	6	23.35,46	5,63
24	18.06.13,2				
		7.49,38	2	7.51,81	2,03
26	17.58.23,42				

820.	Pendulum 1, in Clock 2. Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean Solar day, Temp. 45°,	
	m. s.										
ly 17	5.01,5	s.	s.	s.	s.	°	°	s.	s.		
		49	50,36	50,36	50,498	1.71	47.72	+4,813	+1,186	86456,497	86456,249, or corrected for the buoyancy of the atmosphere, 86462,61.
18	5.53,5										
		305,5	312,01	52,001	52,144	1.588	44,8	+4,151	—0,088	86456,207	
24	10.59										
17	5.13,1										
		49,1	50,64	50,64	50,778	1.707	45,83	+4,796	+0,362	86455,936	86455,889, or corrected for the buoyancy of the atmosphere, 86462,25.
18	6.02,2										
		304,8	310,06	51,677	51,818	1.587	44,81	+4,146	—0,083	86455,881	
24	11.07										
17	5.15,7										
		48,6	50,46	50,46	50,598	1.707	45,54	+4,796	+0,235	86455,629	86455,898, or corrected for the buoyancy of the atmosphere, 86462,259.
18	6.04,3										
		304,8	310,43	51,738	51,88	1.587	44,81	+4,146	—0,083	86455,943	
24	11.09,1										

820.	Pendulum 2, in Clock 1. Fast of 259.	Gain on 259.	Gain on Time.	Gain per diem Sidereal.	Gain per diem Solar.	Mean Arc.	Mean Temp.	Corrections for Arc.	Corrections for Temp.	Vibrations in a mean Solar day, Temp. 45°.	
	m. s.										
ly 17	11.10,2	s.	s.	s.	s.	°	°	s.	s.		
		209,3	210,66	210,66	211,236	0,9	48,34	+1.336	+1.469	86614,041	86614,299, or corrected for the buoyancy of the atmosphere, 86620,672.
18	14.39,5										
		1268,3	1274,81	212,47	213,051	0,892	45,17	+1.312	+0,075	86614,438	
24	35.47,8										
		209,7	210,53	210,53	211,103	0,95	47,97	+1.488	+1,155	86613,746	
25	39.17,5										
17	11.47,7										
		210,4	211,94	211,94	212,52	0,879	46,4	+1.274	+0,615	86614,409	86614,205, or corrected for the buoyancy of the atmosphere, 86620,578.
18	15.18,1										
		1268,1	1273,36	212,227	212,807	0,897	45,24	+1.327	+0,105	86614,239	
24	36.26,2										
		420	422,21	211,1	211,68	0,892	47,32	+1.312	+1,02	86614,012	
26	43.26,2										
17	11.58,2										
		210,5	212,36	212,36	212,941	0,899	46,1	+1.274	+0,484	86614,699	86614,252, or corrected for the buoyancy of the atmosphere, 86620,625.
18	15.28,7										
		1267,8	1273,43	212,238	212,819	0,897	45,28	+1.327	+0,123	86614.269	
24	36.36,5										
		420,2	422,23	211,115	211,695	0,889	47,26	+1.303	+0,994	86613,992	
26	43.36,7										

From the Tables A. and B. are derived the following results.

By Transits of	Vibrations per diem.					
	Pendulum 1, in Clock 2.			Pendulum 2, in Clock 1.		
The Sun, S. of Zen.	Mean of	7 days,	86462,583	Mean of	14 days,	86620,293
The Sun, N. of Zen.		7	86462,236		7	86620,612
Arcturus -		7	86462,61		8	86620,672
α Lyræ -		7	86462,25		9	86620,578
α Aquilæ -		7	86462,259		9	86620,625

Each result being given a value proportioned to the number of days of which it is the average, the mean results are of No. 1 pendulum 86462,3876, and of No. 2 pendulum 86620,523 vibrations; by adding to these numbers the corrections due to the elevation above the sea, being +0,1413 to No. 1, and +0,1416 to No. 2, a final result is obtained; that No. 1 Pendulum vibrating in Clock No. 2 at Melville Island would make 86462,5289 vibrations, and No. 2 Pendulum in Clock No. 1, 86620,6646 vibrations, in a mean solar day, *in vacuo*, at the level of the sea, the temperature being 45°.

The latitude of the spot where the preceding observations were made was 74° 47′ 12,4″, deduced by a mean of 39 meridian altitudes of the sun, observed by Captain Parry and Mr. Beechey, with reflecting circles and sextants with an artificial horizon; the results have been re-computed since the return of the Expedition, using the table of atmospherical refractions, published by Dr. Young in the Nautical Almanack of 1822; the elements of these observations are given in the Appendix to the Narrative of the Voyage, pages lxxxviii and lxxxix.

Results of the preceding operations.

It remains to recapitulate the results which have been detailed, and to state the deductions thereupon.

The acceleration of the pendulum between the stations visited in the first voyage has been already mentioned, viz.

33,107 vibrations between London and Brassa;

32,1316 vibrations between Brassa and Hare Island;

And 65,2386 vibrations between London and Hare Island;

The following Table presents in one view, the results of the four series of observations, by which the acceleration between London and Melville Island has been determined.

		Vibrations.		Acceleration.		
		London.	Melville Island			
Clock 1,	Pendulum 1,	86392,4513	86466,4793	74.028	74.8151	74,734
	Pendulum 2,	86545,0623	86620.6646	75.6023		
Clock 2,	Pendulum 1,	86388,0967	86462,5289	74.4332	74,6529	
	Pendulum 2,	86496,9855	86571,858	74,8725		

It should be remarked, that in the earliest trial which was made of the clocks, No. 2 was considered to be deserving of preference, from its greater precision of beat, occasioned probably in part by the crutch of No. 1 being rather larger than the diameter of the pendulum rods; the above results seem to justify the preference, as the acceleration produced by each of the pendulums vibrating in No. 2, corresponds better than their results when vibrating in No. 1. It will be seen, however, that a mean of the results of either clock separately,

coincides within a tenth of a vibration, with the mean result in the foregoing table.

Assuming then the length of the pendulum vibrating seconds in the latitude of London, viz. 51° 31′ 08,4″ at 39,13929 inches, which has been determined by Captain Kater, the following Table presents its length at each of the stations at which the clocks have been set up, deduced from the observations which have been detailed.

Place of Observation.	Latitude.	Length of the pendulum vibrating seconds.
	° ′ ″	Inches.
London	51.31.08,4 N.	39.13929
Brassa	60.09.42	39.16929
Hare Island	70.26.17	39.1984
Melville Island	74.47.12,4	39.207

Deductions as to the Figure of the Earth.

The following Table contains the diminution of gravity from the Pole to the Equator, and the resulting ellipticity of the earth deduced from the preceding observations.

The method which has been followed in obtaining these deductions, is the same which has been described by Captain Kater in the Philosophical Transactions for 1819, p. 420, and 421.

	Diminution of Gravity.	Ellipticity.
London and Brassa	·0055066	$\frac{1}{314,3}$
London and Hare Island	·0055082	$\frac{1}{314,2}$
Brassa and Hare Island	·0055139	$\frac{1}{313,6}$
London and Melville Island	·0055258	$\frac{1}{312,6}$

In concluding the account of these experiments, it is proper that I should notice, that their success is in great measure to be attributed to Mr. BROWNE and to Captain KATER: to Captain KATER for his care and judgment in preparing the instruments, and to Mr. BROWNE for permitting the clocks to be set up in his house in London, and for the advantage of comparison with his excellent timepieces.

I would also avail myself of this occasion, to express my personal obligations to those Gentlemen, for the opportunity which I have enjoyed of conducting these experiments; and for which I am sensible that I am chiefly indebted to the statement they were pleased to make, of my competency to fulfil the purposes which the Society had in view.

XV. *Some Observations and Experiments on the Papyri found in the ruins of Herculaneum. By Sir* HUMPHRY DAVY, *Bart. P. R. S.*

Read March 15, 1821.

IN a paper intended for private circulation only on the MSS. found in the excavations made at Herculaneum, but which was published, by mistake, in the Journal of Science and the Arts, I have described, in a general manner, the circumstances which led me to make experiments on these remains, and mentioned some of my first observations on this subject. Mr. HAMILTON, to whom this communication was sent, entered into my views with all that ardour for promoting the progress of useful knowledge which so peculiarly belongs to his character, and on his representation of them, the Earl of LIVERPOOL and Viscount CASTLEREAGH, with the greatest liberality, placed at my disposal such funds as were requisite for paying the persons whom it was necessary to employ in trying new chemical methods of unrolling the MSS. and for examining and preserving them when unrolled; and his present MAJESTY, then PRINCE REGENT, graciously condescended to patronize the undertaking.

In this communication, I shall do myself the honour of laying before the Royal Society an account of all that I have been able to do on this subject; namely, first, a detail of my early experiments in England on fragments of papyri, which induced me to believe that chemistry might afford considerable assistance towards unrolling the MSS. Secondly,

a description of the rolls in the Museum at Naples, and of some analytical experiments I made upon them. Thirdly, a detail of the various chemical processes carried on in the Museum at Naples on the MSS., and of the reasons which induced me to renounce my undertaking before it was compleated. And lastly, some general observations on the MSS. of the ancients.

I trust these matters will not be found wholly devoid of interest by the Society, and that they will excuse some repetitions of what I have stated in the report before referred to, as they are necessary for a complete elucidation of the subject.

1st. *An account of some experiments made in England on fragments of papyri in* 1818.

In examining, chemically, some fragments of a roll of papyrus found at Herculaneum, the leaves of which adhered very strongly together, I found that it afforded, by exposure to heat, a considerable quantity of gaseous matter, which was principally inflammable gas, and when acted on by muriatic or nitric ether, it coloured them; and when it was exposed to heat after the action of these fluids, there was an evident separation of the leaves of the MS.

Chlorine and iodine, it is well known, have no action upon pure carbonaceous substances, and a strong attraction for hydrogen; and it occurred to me, that these bodies might with propriety be used in attempting to destroy the matter which caused the adhesion of the leaves, without the possibility of injuring the letters on the papyri, the ink of the ancients, as it is well known, being composed of charcoal.

Having through the polite assistance of Sir Thomas Tyrwhitt procured some fragments of papyri on which Dr. Sickler, and some on which Dr. Hayter had operated, and by the kindness of Dr. Young a small portion of a MS. which he had himself unsuccessfully tried to unroll, I made some experiments upon them, by exposing them to the action of chlorine and the vapour of iodine, heating them gently after the process. These trials all afforded more or less hopes of success. When a fragment of a brown MS. in which the layers were strongly adherent, was placed in an atmosphere of chlorine, there was an immediate action, the papyrus smoked and became yellow, and the letters appeared much more distinct; and by the application of heat the layers separated from each other, giving off fumes of muriatic acid. The vapour of iodine had a less distinct action, but still a sensible one; and it was found that by applying heat alone to a fragment in a close vessel filled with carbonic acid or the vapour of ether, so as to raise the heat very gradually, and as gradually to lower it, there was a marked improvement in its texture, and it was much more easily unrolled.

Even in these preliminary trials, I found that it was necessary to employ only a limited and small quantity of chlorine, too large a quantity injuring the texture of the layer, and decomposing the earths which it contained; and that the action of heat was much more efficacious when the MS. had previously been exposed to chlorine, as the muriatic acid vapour formed greatly assisted the separation of the leaves, and a smaller degree of heat was required. But in all the trials, I found the success absolutely depended upon the manner in which the temperature was regulated. When the

fragment was too rapidly heated, the elastic fluid disengaged usually burst the folds of the MS. and when the heat was lowered too suddenly, the layers sometimes split in irregular parts, probably from the sudden contraction consequent on quick cooling.

From the products of the distillation of these fragments, which were water, acetous acid, ammonia, carbonic acid, and much inflammable gas, I inferred, that the papyri to which they belonged must contain much undecomposed vegetable matter, and could not be purely carbonaceous; but as there were great differences in the appearances even of the few papyri in England, which had been presented to his Majesty GEORGE IV. when PRINCE OF WALES, an opinion on this subject was more likely to be correct when formed after an examination not only of all the MSS. found at Herculaneum, but likewise of the circumstances of the excavations made there; and I had an opportunity, during the time I remained at Naples, in two successive winters, to satisfy my mind on this subject, and to obtain the information which will be given in the next Section.

2dly. On the state of the MSS. found at Herculaneum.

The persons who have the care of the MSS. found at Herculaneum, state that their original number was 1696, and that 431 have been operated upon or presented to foreign governments, so that 1265 ought to remain; but amongst these, by far the larger proportion are small fragments, or specimens so injured and mutilated, that there is not the least chance of recovering any portion of their contents; and when I first examined the rolls in detail in

January, 1819, it did not appear to me, that more than from 80 to 120 offered proper subjects for experiments; and this estimate, as my researches proceeded, appeared much too high. These MSS. had been objects of interest for nearly 70 years; the best had long ago been operated upon, and those remaining had not only undergone injuries from time, but likewise from other causes, such as transport, rude examination, and mutilations for the purpose of determining if they contained characters.

The appearances of different rolls were extremely various. They were of all shades of colours from a light chesnut brown to a deep black; some externally were of a glossy black, like jet, which the superintendants called "varnished;" several contained the umbilicus or rolling stick in the middle converted into dense charcoal. I saw two or three specimens of papyri which had the remains of characters on both sides, but in general one side only was written upon. In their texture they were as various as in their colours; the pale brown ones in general presented only a kind of skeleton of a leaf, in which the earthy matter was nearly in as large a proportion as the vegetable matter, and they were light, and the layers easily separated from each other. A number of darker brown ones which, from a few characters discovered in opening them, appeared to be Latin MSS., were agglutinated as it were into one mass; and when they were opened by introducing a needle between the layers, spots or lines of charcoal appeared where the folds had been, as if the letters had been washed out by water, and the matter of which they were composed deposited on the folds. Amongst the black MSS. a very few fragments

presented leaves which separated from each other with considerable facility, and such had been for the most part operated upon; but in general the MSS. of this class were hard, heavy, and coherent, and contained fine volcanic dust within their folds. Some few of the black and darker brown MSS., which were loose in their texture, were almost entirely decayed, and exhibited on their surface a quantity of brown powder.

The persons to whom the care of these MSS. is confided, or who have worked upon them, have always attributed these different appearances to the action of fire, more or less intense, according to the proximity of the lava, which has been imagined to have covered the part of the city in which they were found; but this idea is entirely erroneous, that part of Herculaneum being, as I satisfied myself by repeated examinations, under a bed of tufa formed of sand, volcanic ashes, stones, and dust, cemented by the operation of water (probably at the time of its action in a boiling state). And there is great reason to conclude, that the different states of the MSS. depend upon a gradual process of decomposition: the loose chesnut ones probably not having been wetted, but merely changed by the re-action of their elements, assisted by the operation of a small quantity of air; the black ones, which easily unroll, probably remained in a moist state without any percolation of water; and the dense ones, containing earthy matter, had probably been acted on by warm water, which not only carried into the folds earthy matter suspended in it, but likewise dissolved the starch and gluten used in preparing the papyrus and the glue of the ink, and distributed them through the substance of the MSS., and some of these rolls

had probably been strongly compressed when moist in different positions.

The operation of fire is not at all necessary for producing such an imperfect carbonization of vegetable matter as that displayed by the MSS: Thus, at Pompeii, which was covered by a shower of ashes that must have been cold, as they fell at a distance of seven or eight miles from the crater of Vesuvius, the wood of the houses is uniformly found converted into charcoal; yet the colours on the walls, most of which would have been destroyed or altered by heat, are perfectly fresh, and where papyri have been found in these houses, they have appeared in the form of white ashes, as of burnt paper; an effect produced by the slow action of the air penetrating through the loose ashes, and which has been impeded or prevented in Herculaneum by the tufa, which, as it were, has hermetically sealed up the town, and prevented any decay, except such as occurs in the spontaneous decomposition of vegetable substances, exposed to the limited operation of water and air; for instance, peat and Bovey coal.

The results of the action of heat upon the different specimens of the papyri, proved likewise, that they had never before been exposed to any considerable degree of temperature.

Various specimens of papyri were heated to dull redness in a small covered crucible of platinum to which air had no access. Some of the chesnut and most perfect specimens lost nearly half their weight, and the very black ones, and those containing the largest quantity of white ashes, all lost more than one-third, as the following results, selected from a number, will show:

No. 1. 100 parts of a pale chesnut papyrus lost 45 parts.
No. 2. 100 parts of a decomposed papyrus, chesnut-coloured, but darker, lost - - 43.
No. 3. 100 parts of a very black papyrus lost 42.
No. 4. 100 parts of a pale papyrus, extremely loose in texture and partly converted into white ashes, lost - - - 41.
No. 5. 100 parts of another of the same kind lost 38.

When the whole of the carbonaceous and vegetable matter of the papyrus was destroyed by slow combustion, the white ashes remaining, which were principally carbonate of lime and lime, proved to be from $\frac{1}{16}$ to $\frac{1}{20}$ of the original weight of the papyrus; and in those specimens which were most dense, and that contained a white powder, the proportion of ashes was greater, and a larger quantity was insoluble in acids.

Ammonia was found in the products of all the papyri that I distilled, but least in those which contained no distinct characters; from which it is probable that it arose principally from decomposed glue used in the manufacture of the ink, and which had been principally dissolved and carried off in those papyri which had been most exposed to the action of water.

I ascertained, that what the Neapolitans called varnish, was decomposed skin, that had been used to infold some of the papyri, and which by chemical changes had produced a brilliant animal carbonaceous substance; this substance afforded abundance of ammonia by distillation, and left ashes containing much phosphate of lime.

3dly. An account of the experiments on Papyri, made in the Museum at Naples.

Only one method, and that a very simple mechanical one, has been adopted for unrolling the MSS. It was invented by Padre Piaggi, a Roman, and consists in attaching thin animal membrane by a solution of glue to the back of the MSS. and carefully elevating the layers by silk threads when the glue is dry.

In considering this method in its general application, some circumstances occurred to me which afforded an immediate improvement. A liquid solution of glue had been used, which, when the texture of the MSS. was loose or broken, penetrated through three or four layers, and these, when the glue dried, separated together. To obviate this objection, I mixed the solution of glue with a sufficient quantity of alcohol to gelatinize it; and a mixture of the jelly and the fluid being made and applied by a camel's hair brush, a film of jelly remained on the exterior of the surface of the leaf, which attached itself to the membrane.

The effect of the solution of glue applied in the ancient method, was always likewise to separate the layers, by expanding the imperfectly carbonized fibres. In the improvement I have mentioned, the alcohol, from its greater lightness, penetrated farther into the papyrus, but produced its greatest effect immediately on the first layers.

I adopted in some cases ether, as an agent for assisting the separation of the layers; and it was always found very efficacious, whether it was necessary to remove a single

layer, or several layers at a time, in order to discover if a roll contained characters. The ether was applied by a camel's hair brush lightly to the surface of the leaf, when its operation was intended to be merely on that leaf; and it was suffered to sink deeper according as more layers were to be separated; the mere circumstances of its evaporation, which in some cases I assisted by heat, tended to detach the layers. For the black MSS. I employed sulphuric ether, and for the brown ones muriatic or nitric ether in their impure states, *i. e.* mixed with much alcohol.

No artificial modes had been employed by the Neapolitans for drying the papyrus in the operation of attaching the membrane, and no means, except mechanical ones, of detaching it after it was dried.

By throwing a stream of air gradually warmed till it attained a temperature about that of boiling water upon the surface of the leaf, I succeeded not only in drying the layers with much greater rapidity, but likewise in separating them with more delicacy.

I tried different modes of heating the air to be thrown upon the papyrus, such as passing it in a spiral metallic tube through warm water or oil by a double bellows, and from a large bladder through a straight tube having a very fine orifice, and heated by a copper ball surrounding the body of the tube, and exposed to burning charcoal; which last method, from its simplicity, I found the one best fitted to the Neapolitan operators. By sending the stream of air from a greater or smaller distance, so that it mixed with more or less cold air, the degree of temperature applied was regulated at pleasure. It was always found necessary to suffer a few minutes to elapse after

the membrane was attached, and then to begin with a very slight increase of temperature; as otherwise, by too sudden an application of heat, the membrane shrivelled before it became adherent, and the vapour suddenly raised destroyed its union with the papyrus; whereas, when the moisture was suffered to drain from the gelatinized glue, and the temperature was gradually raised, the expansion of the skin and the upper layer separated them perfectly from the lower layers, so that the unrolling was performed, as it were, by chemical means; and an operation, which hitherto had required some hours for its completion, was easily effected in from 30 to 40 minutes.

I tried several experiments, by substituting solution of resins in alcohol and of gums in water for the gelatinized solution; but none of them answered so well; the resins would not adhere with any tenacity to the membrane, and the gums, when dried, had not that flexibility, which is an important character in the glue.

The alterations in the mode of applying and drying the membrane used to detach and preserve the leaves of MSS. capable of being unrolled, applied generally; I shall now mention the plans I adopted for the preparation of the MSS. for this operation.

MSS. in different states required a treatment of a directly opposite kind, which was to be modified according to circumstances. The pale chesnut-coloured MSS., covered partially with white ashes, were generally of a texture so loose, and had their layers so destroyed, that there was considerable danger of their falling into pieces by mere touching. The characters that remained in many of them were extremely distinct; and when a number of layers were taken up at

once, it appeared as if they presented perfect columns of writing; but the fact is, the papyrus was full of holes, and each line was made up of letters from several different folds of the MS. When the process of unrolling these papyri was performed in the common way, the result obtained appeared, till it was examined minutely, a perfect column; but was in fact made up of the letters of different words. I endeavoured to obtain the fragments of a single leaf attached to a layer of membrane by applying a solution of caoutchouc in ether to the surface of a MS., so as to supply the parts of the leaf destroyed; but operating in this way, I obtained only a few characters, and never an entire word; so that after various unsuccessful trials, I was obliged to give up the MSS. of this description as hopeless; more than $\frac{5}{6}$ of their contents probably being always destroyed, and that in so irregular a way as to leave no entire sentences, or even words.

On two brown MSS., which were firm in their texture, and had the appearance of peat, and the leaves of which would not separate by common means, I tried the experiment of heating, after they had absorbed a small quantity of chlorine; and I found that in both cases the leaves detached themselves from each other, and were easily unrolled; but these MSS. had been so penetrated by water, that there were only a few folds which contained words, and the letters were generally erased, and the charcoal which had composed them was deposited on the folds of the MSS.

Of the black MSS., of which the layers were perfect and easily separated, all the best specimens had been unrolled or operated upon, so that fragments only of this description

remained. By assisting the operation of detaching the layers by muriatic ether and the other processes mentioned in page 199, many parts of columns were obtained from several of the fragments, by which some idea of their contents may be formed.

On the black compact and heavy MSS. which contained white earthy matter in their folds, I tried several experiments, with the hopes of separating them into single layers, both by the action of muriatic and nitric ether, and by the operation of chlorine and of weak hydrofluoric acid, assisted by heat; but generally the fibres of the papyrus had been so firmly cemented together, and so much earthy matter had penetrated them, that only a very imperfect separation could be obtained, and in parts where vestiges only of letters appeared, so that from MSS. of this kind only a few remains of sentences could be gained.

During the two months that I was actively employed in experiments on the papyri at Naples, I had succeeded, with the assistance of six of the persons attached to the Museum, and whom I had engaged for the purpose, in partially unrolling 23 MSS., from which fragments of writing were obtained, and in examining about 120 others, which afforded no hopes of success; and I should gladly have gone on with the undertaking, from the mere prospect of a possibility of discovering some better results, had not the labour, in itself difficult and unpleasant, been made more so, by the conduct of the persons at the head of this department in the Museum. At first every disposition was shown to promote my researches; for the papyri remaining unrolled were considered by them as incapable of affording any thing legible by the

former methods, or, to use their own word, *disperati;* and the efficacy and use of the new processes were fully allowed by the Svolgatori or unrollers of the Museum; and I was for some time permitted to choose and operate upon the specimens at my own pleasure. When, however, the Reverend Peter Elmsley, whose zeal for the promotion of ancient literature brought him to Naples for the purpose of assisting in the undertaking, began to examine the fragments unrolled, a jealousy, with regard to his assistance, was immediately manifested; and obstacles, which the kind interference of Sir William A'Court was not always capable of removing, were soon opposed to the progress of our enquiries; and these obstacles were so multiplied, and made so vexatious towards the end of February, that we conceived it would be both a waste of the public money, and a compromise of our own characters, to proceed.

4thly. *Some general observations.*

The Roman MSS. found in the Museum, are in general composed of papyrus of a much thicker texture than the Greek ones, and the Roman characters are usually larger, and the rolls much more voluminous; the characters of the Greek MSS., likewise, with a few exceptions, are more perfect than those of the Latin ones.

From the mixture of Greek characters in several fragments of Latin MSS., and from the form of the letters and the state of decomposition in which they are found, it is extremely probable that they were of a very ancient date when buried.

I looked in vain amongst the MSS. and on the animal charcoal surrounding them, for vestiges of letters in oxide of

iron; and it would seem from these circumstances, as well as from the omission of any mention of such a substance by Pliny, that the Romans, up to his period, never used the *ink of galls and iron* for writing: and it is very probable, that the adoption of this ink, and the use of parchment, took place at the same time. For the ink composed of charcoal and solution of glue can scarcely be made to adhere to skin; whereas the free acid of the chemical ink partly dissolves the gelatine of the MSS., and the whole substance adheres as a mordant; and in some old parchments, the ink of which must have contained much free acid, the letters have, as it were, eaten through the skin, the effect being always most violent on the side of the parchment containing no animal oil.

The earliest MSS. probably in existence on parchment, are those codices rescripti, discovered by Monsignore MAI, in the libraries of Milan and Rome. Through his politeness I have examined these MSS., particularly that containing some of the books of Cicero de Republica, and which he refers to the second or third century. From the form of the columns, it is very probable that they were copied from a papyrus. The vegetable matter which rendered the oxide of iron black is entirely destroyed, but the peroxide of iron remains; and where it is not covered by the modern MSS., the form of the letter is sufficiently distinct. Monsignore MAI uses solution of galls for reviving the blackness. I have tried several substances for restoring colour to the letters in ancient MSS. The triple prussiate of potash, used in the manner recommended by the late Sir CHARLES BLAGDEN, with the alternation of acid, I have found successful; but by making a weak solution of it with a small quantity of muriatic acid, and by applying them to the letters in their state of

mixture with a camel's hair pencil, the results are still better.

It is remarkable, that no fragments of Greek, and very few only of Latin poetry, have been found in the whole collection of the MSS. of Herculaneum; and the sentences in the specimens we unrolled, in which Mr. ELMSLEY was able to find a sufficient number of words to infer their meaning,* show that the works, of which they are the remains, were of the same kind as those before examined, and belonged to the schools of the Greek Epicurean philosophers and sophists.

Nearly 1000 columns of different works, a great part unrolled under the superintendance of Mr. HAYTER, and at the expense of his present Majesty GEORGE IV. have been copied and engraved by the artists employed in the Museum; but from the characters of the persons charged with their publication, there is very little probability of their being, for many years, offered to the world; which is much to be regretted; for though not interesting from their perfection as literary works, they would unquestionably throw much light upon the state of civilization, letters and science, of the age and country to which they belonged.

Should discoveries of MSS. at any future time be made at Herculaneum, it is to be hoped that the papyri will be immediately excluded from the atmosphere, by being put into air-tight cases, filled with carbonic acid after their introduction. There can be no doubt that the specimens now in the Museum, were in a much better state when they were first discovered; and the most perfect even, and those the coarsest in their texture, must have been greatly injured during the 69

* Engravings of copies of a few of these fragments, selected from nearly 100, are annexed to this paper, for the purpose of showing their nature.

years that they have been exposed to the atmosphere. I found that a fragment of a brown MS. kept for a few weeks in a portion of air confined by mercury, had caused the disappearance of a considerable part of the oxygene, and the formation of much carbonic acid.

Plate XI.

Fig. 1. represents a papyrus partly unrolled, with the ink-stand and reed for writing used by the ancients.

Fig. 2. represents a box of papyri; both copied from the "Pitture antiche d'Ercolano."

Plate XII.

Fig. 1. is a specimen of an unrolled papyrus, which is so destroyed, that the letters of different columns appear through the folds, as if they formed one column.

Figs. 2 and 3. are specimens of fragments, in which the lines begin with Greek capitals.

Plate XIII.

Contains a specimen of a fragment of a Roman MS. of which the characters are partly Greek.

PLATE XIV.

Contains specimens of fragments of a Greek MS.

PLATE XV.

Contains specimens of fragments of another Greek MS.

PLATE XVI.

Contains specimens of fragments of a MS. in Roman capitals.

PLATE XVII.

Contains specimens of MSS. supposed to be Roman, written in peculiar characters.

PLATE XVIII.

Fig. 1. Specimen of a fragment of a MS., of which the characters have not been yet examined.

Fig. 2. Specimen of a fragment of a Greek MS.

Fig. 1.

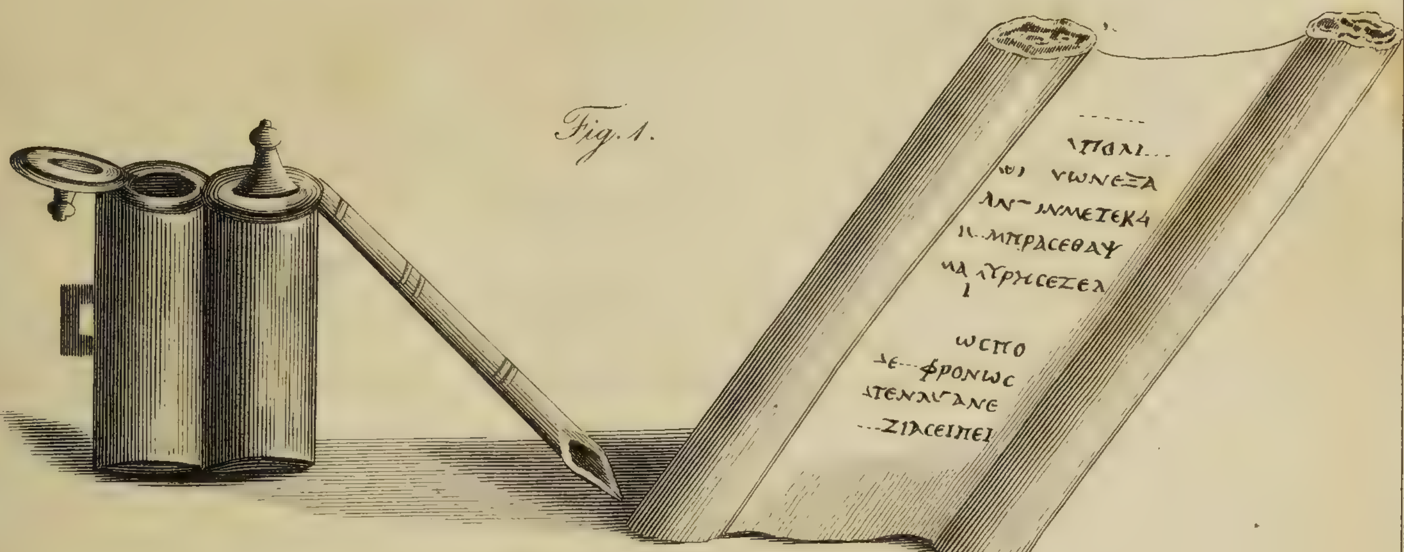
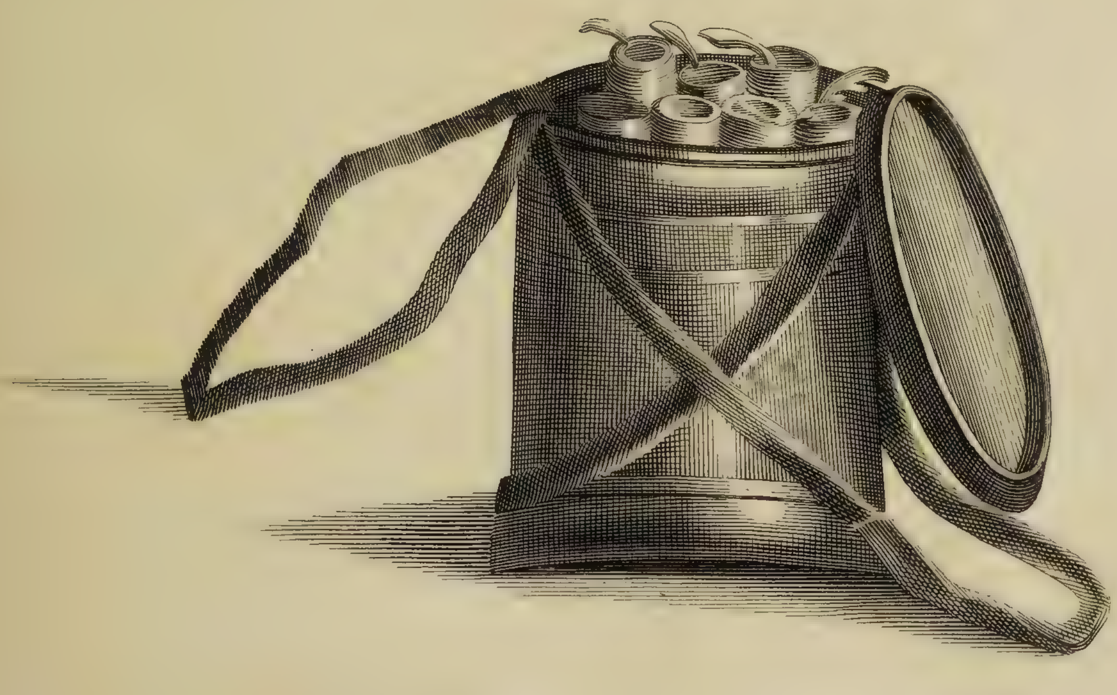


Fig. 2.



Jas. Basire sc.

Fig. 1.

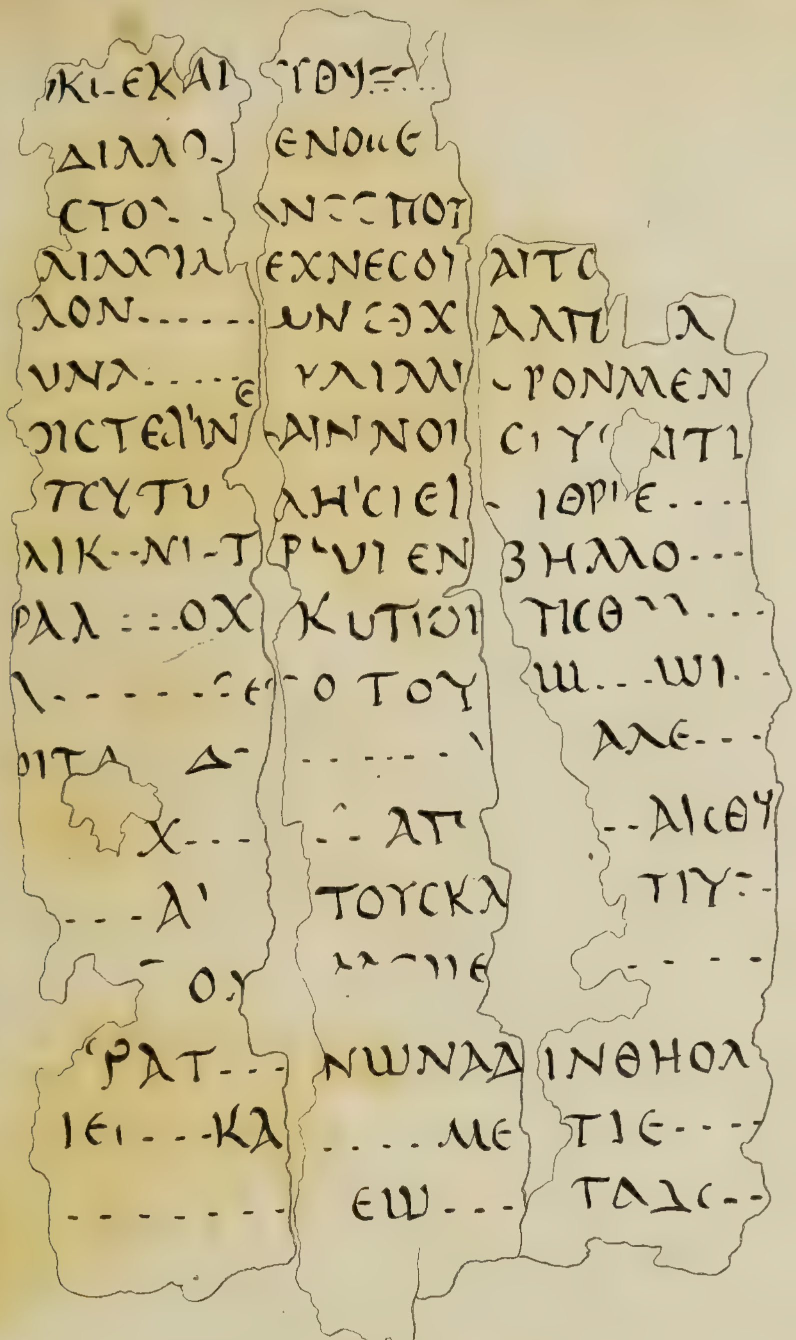


Fig. 2.

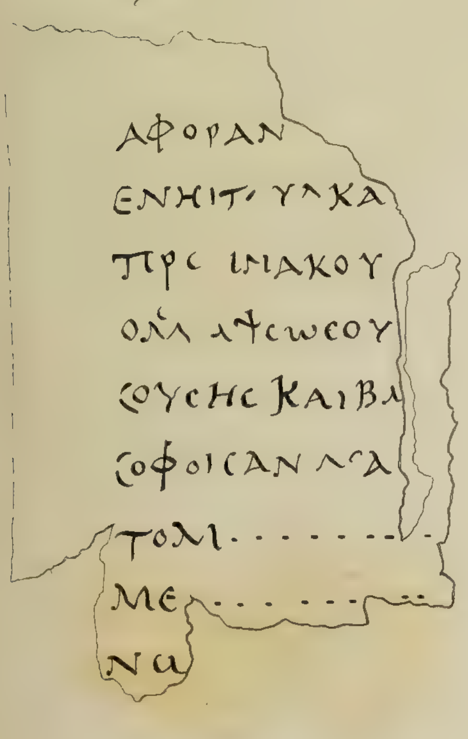
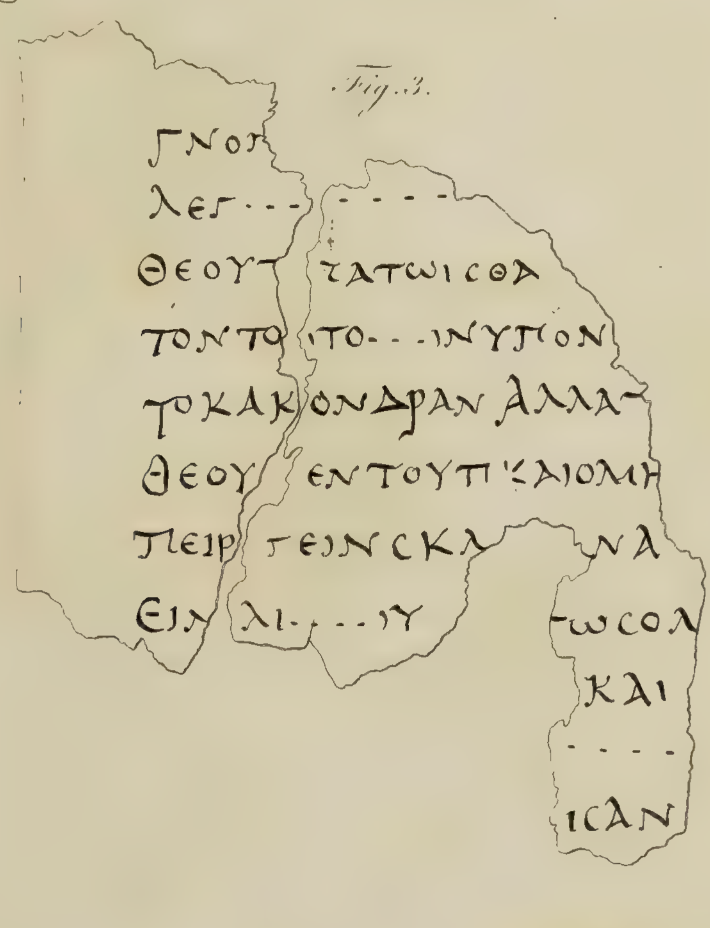
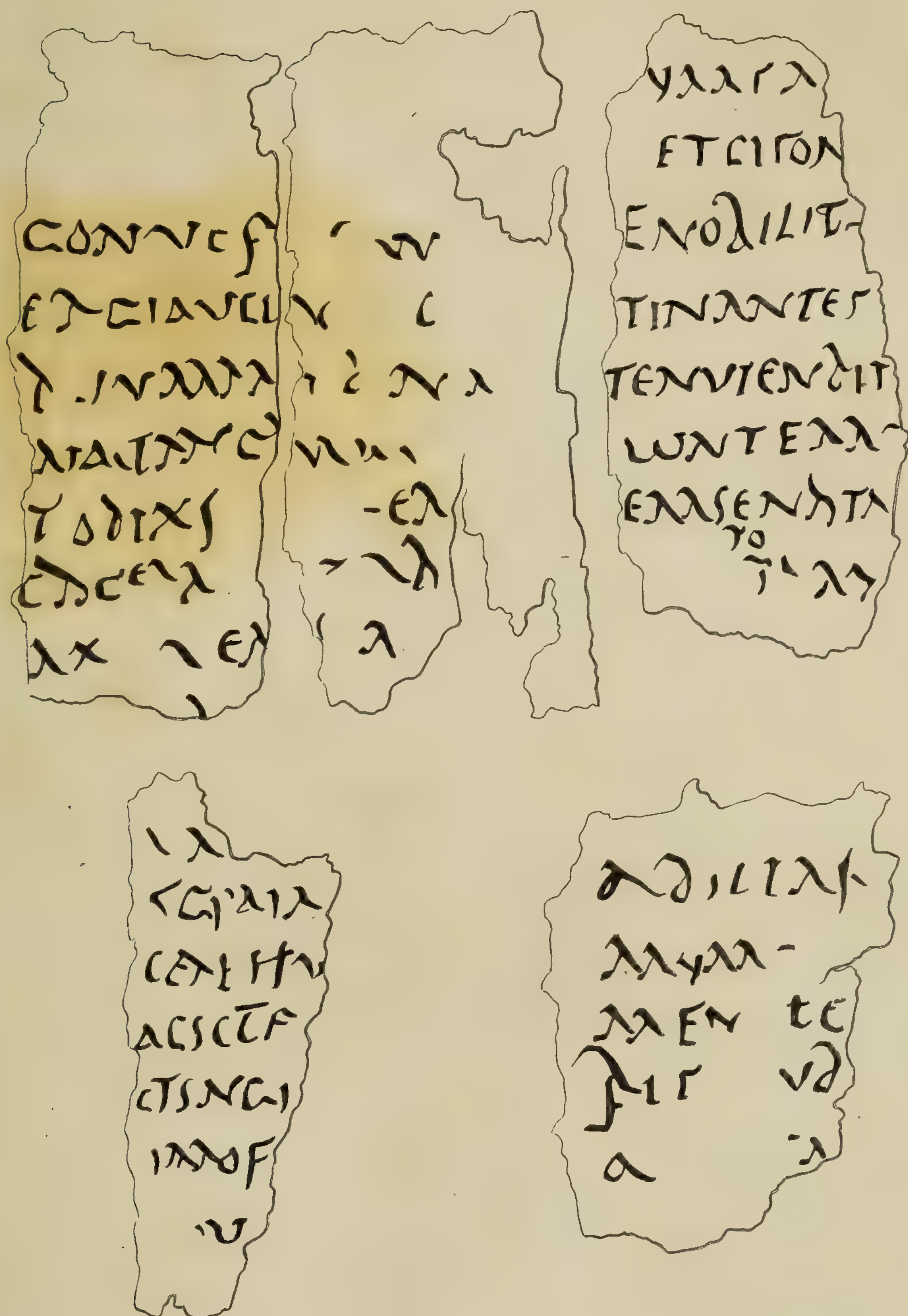


Fig. 3.





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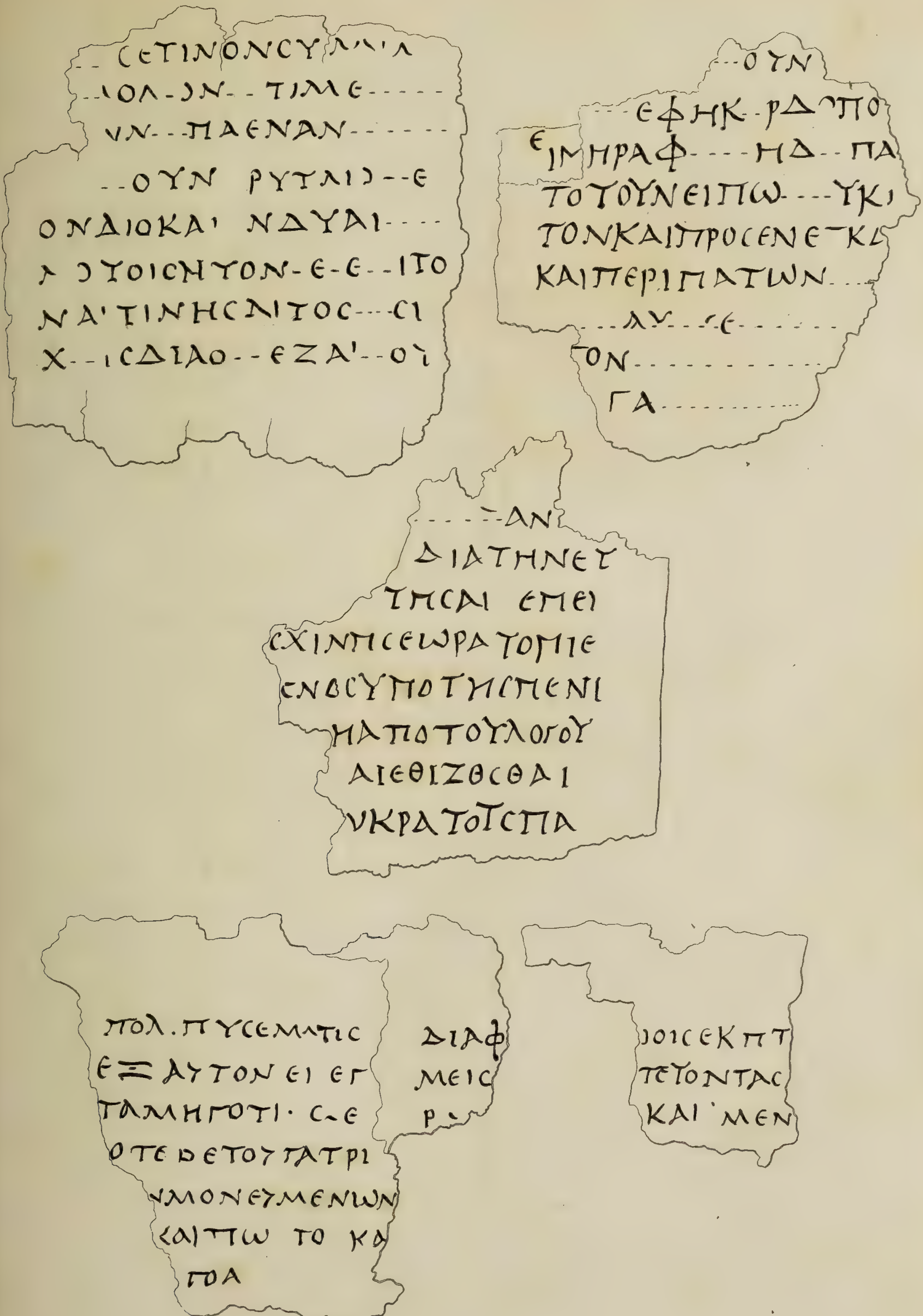
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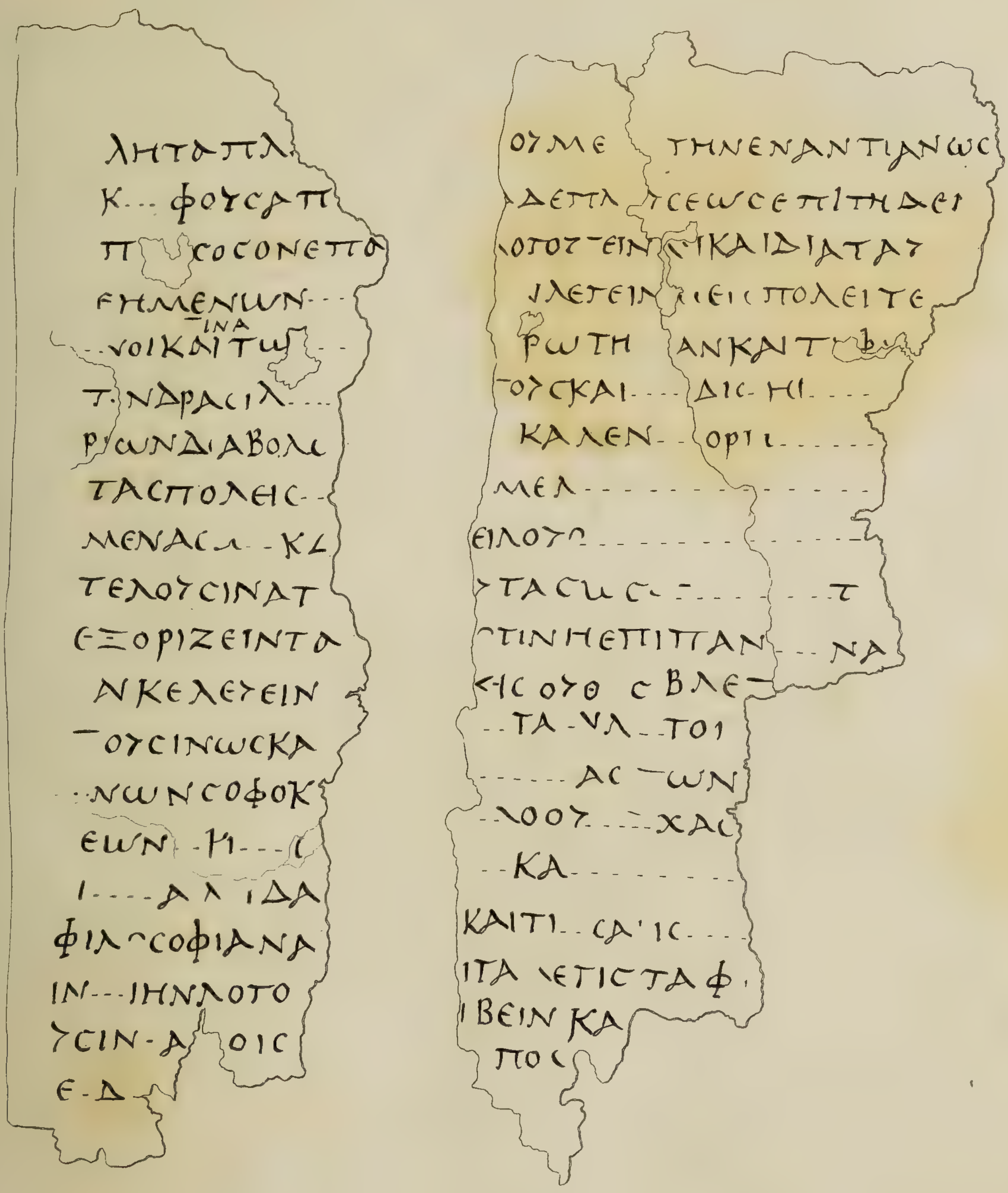

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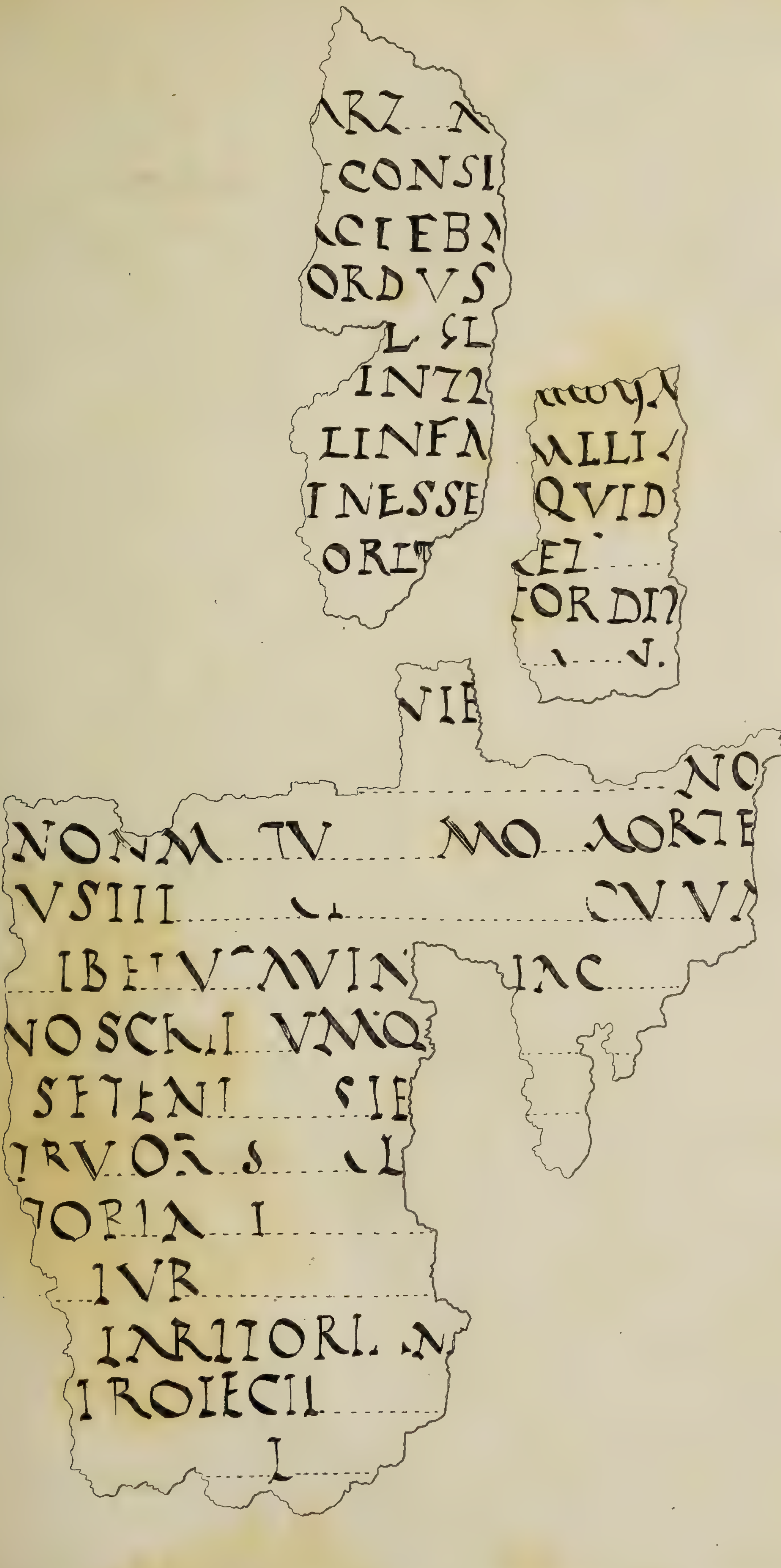

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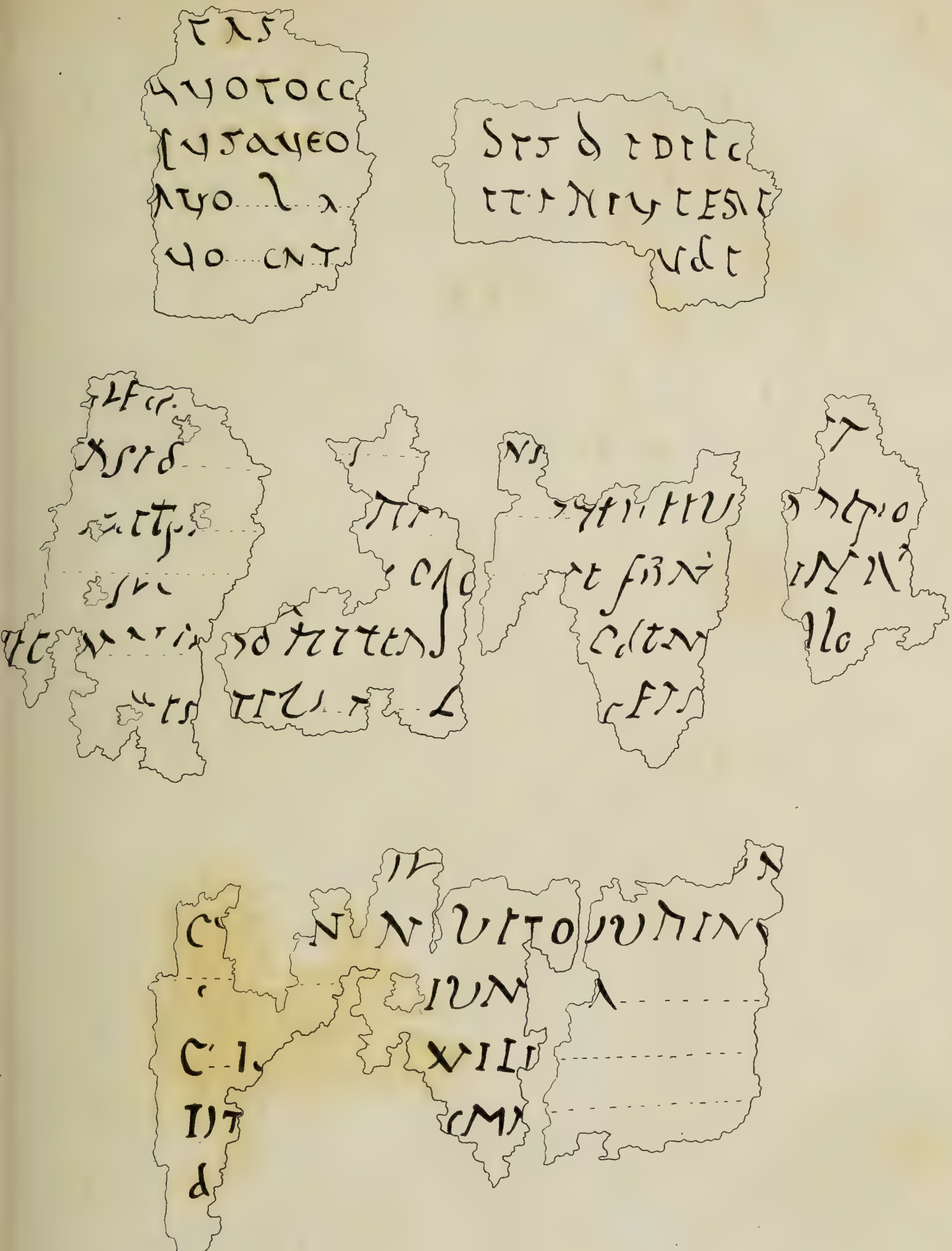

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Fig. 1.

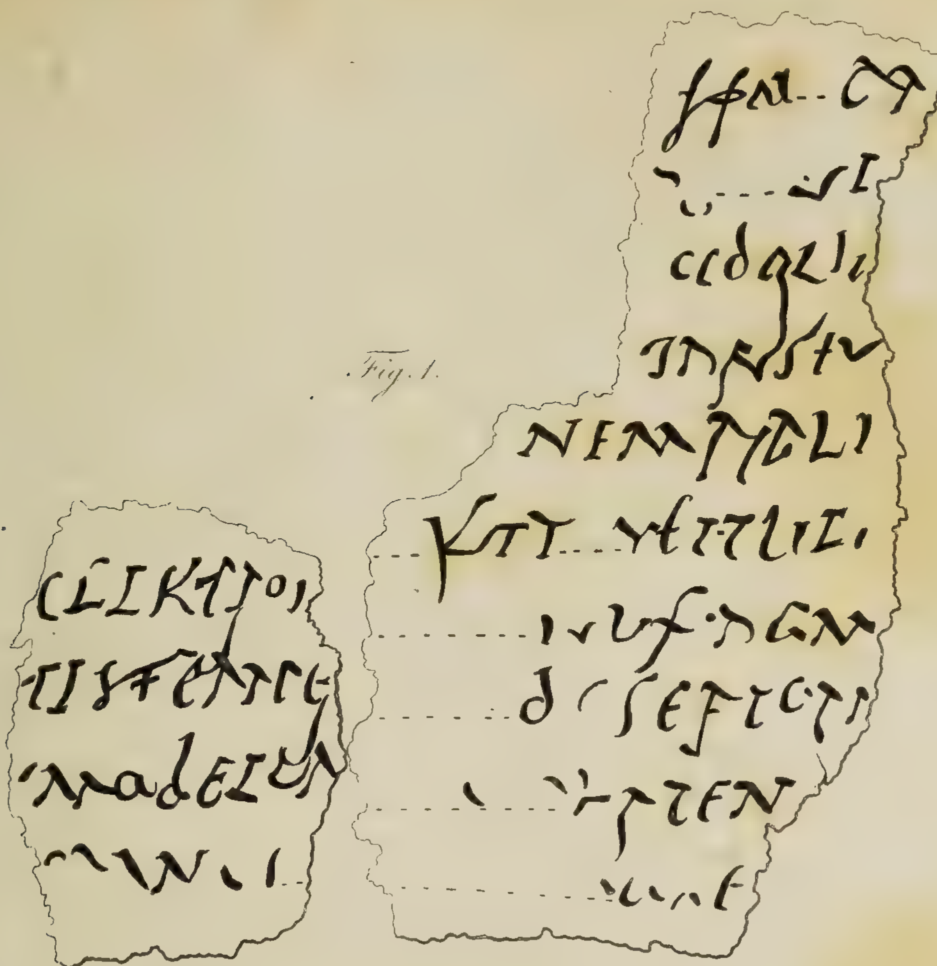
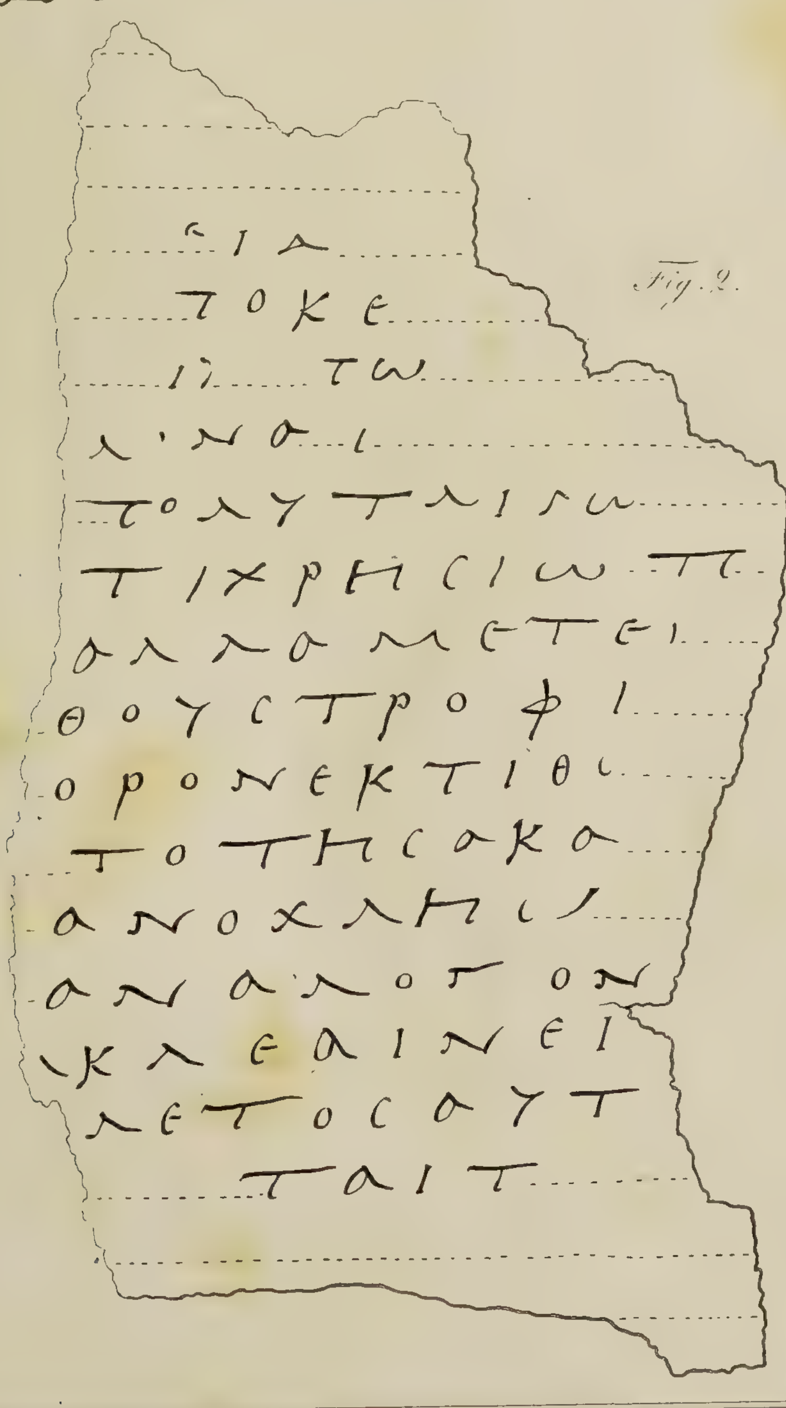


Fig. 2.



J. Basire sc.

XVI. *Observations on Naphthaline, a peculiar substance resembling a concrete essential oil, which is apparently produced during the decomposition of coal tar, by exposure to a red heat. By* J. Kidd, *M. D. Professor of Chemistry, Oxford. Communicated by* W. H. Wollaston, *M. D. F. R. S.*

Read March 8, 1821.

Although the existence, and many of the properties of the substance above-mentioned, have been already noticed in two of the Philosophical Journals of this country,* there has not yet appeared, as far as I can discover, any systematic description of the mode by which it may be obtained, or of its relation to the substance from which it is produced; on which account I have been induced to offer to the Royal Society the following observations respecting these points of its history.

In the experiments which led, in the present instance, to the detection of the substance in question, it was proposed to effect the decomposition of coal tar by passing its vapour through an ignited iron tube; and, in order to increase to the utmost the extent of the ignited surface, that portion of the tube which was constantly kept up to a red heat, was filled, in the first instance, with a series of hollow iron cylinders open at both extremities, and successively decreasing in diameter, so as to be included one within another. In

* Thomson's Annals of Philosophy, January, 1820, page 74; and Mr. Brande's Quarterly Journal, January, 1820, page 287.

other instances these cylinders were removed, and their place supplied by sand, or by pieces of well burnt coke, or by pieces of brick; but it was found that the interstices between the cylinders, or between the particles of sand, &c. were so soon choaked up with carbon from the decomposition of the tar, as to be rendered absolutely impervious to the gas produced during the decomposition; so that it became necessary to pass the vapour of the tar simply through the tube itself.

Connected with the tube in which the tar was decomposed was a vessel, in which any undecomposed vapour of the tar, or any products resulting from its decomposition, might be condensed; and at the end of every experiment this condensing vessel was found to contain an aqueous fluid having an ammoniacal odour, and a dark coloured liquid, resembling tar in appearance.

This dark coloured liquid is characterized by the following properties:

Its colour, in the mass, is black; but when spread in a thin stratum on paper or glass, it is of a clear deep reddish brown colour.

It is a much thinner liquid than the coal tar from which it was produced; and has a peculiar and slightly aromatic odour, together with the smell of ammonia; about three-fourths of a given quantity of it pass through unsized paper; and that which remains on the paper resembles common tar. Sp. gr. 1050; the sp. gr. of the tar from which it was produced being 1109.

Readily and entirely soluble in ether.

Soluble, but not entirely, in alcohol; the solution becoming

milky upon the addition of water, and this milky mixture passing unaltered through the pores of the closest filtering paper.

Not miscible with water; but readily communicating to it a light brown colour, and a taste at first sweet, but followed by an aromatic pungency. The water acquires alkaline properties, and holds ammonia in solution. When poured out on a flat surface, it catches fire almost immediately on the application of flame, and burns for a time exactly in the same manner as a thin stratum of alcohol, the flame being blue and lambent, and without smoke; but after a few seconds the flame becomes white, and the liquid begins to burn with much black smoke, and with a crackling noise.

A pint of this dark coloured liquid was submitted to very slow distillation in a large glass retort connected with a large glass receiver, from the interior of which all communication with the external air was excluded by means of a common safety valve. The heat was supplied from the flame of an Argand gas burner, and was so slight as scarcely to inconvenience the naked hand, when held over it immediately under the bottom of the retort.

The same degree of heat was applied constantly during forty hours; at the end of which time there had distilled into the receiver rather more than half a pint of a liquid, which consisted of two perfectly distinct portions, which, however, had uniformly passed over together from the very commencement of the distillation.

The uppermost of these portions, in appearance, resembled pale olive oil, and amounted to not quite a quarter of a pint. The lowermost portion resembled water, but was not

perfectly transparent, and amounted to rather more than a quarter of a pint: but there is ground for believing, from the results of subsequent distillations, that the proportion of the aqueous product is variable; and that it is greater when the distillation is carried on slowly, than when it is carried on rapidly.

After the above-mentioned products had passed over, a concrete substance as white as snow began to collect in dispersed crystalline flocculi, in the upper part of the body and neck of the retort, so as in a short time almost wholly to obstruct the passage; the oily fluid and the water continuing to pass over at the same time, but much more slowly than before.

At the end of sixty hours the original quantity of the dark coloured liquid was reduced to about a quarter of a pint; and what remained was much thickened in consistence: the heat was therefore increased: and now there began to pass over a darker coloured and thicker oil, which, as it advanced farther from the source of heat, congealed into a substance of the consistence of butter. The heat being still more increased, this oil became darker coloured and more dense; and when at the last there remained in the retort not above one-eighth of the quantity originally poured into it, and the heat of the gas burner had been increased to the utmost, there arose a heavy yellow vapour, which was condensed in the neck of the retort in the form of a farina of a bright yellow colour.

When it appeared that the heat no longer separated any thing from the black matter in the retort, which still however retained a degree of fluidity, the apparatus was suffered to

cool; during which time the residuum became fixed, and to the eye resembled pitch.

The several products of the distillation above described being carefully separated from each other, the more remarkable of them were submitted to examination; but as leisure was wanting for a full investigation of their characters, the Society is requested to accept, with some indulgence, the following description of such of their properties as were ascertained.

Properties of the aqueous product.

Taste, saline and alkaline; with an ammoniacal and slightly aromatic odour.

Sp. gr. 1023.

Became faintly blue by the addition of a solution of prussiate of potash.

Grs. 700 of this aqueous fluid were evaporated under an exhausted receiver inclosing a quantity of dry muriate of lime: the residuum of the 700 grains weighed not more than half a grain, and consisted partly of a brown oil and partly of a sparingly soluble saline matter, which by the proper tests was found to contain sulphuric acid and muriatic acid; the former apparently in greater quantity than the latter.

Properties of the oily fluid.

Taste, pungent, bituminous, and aromatic; with an odour similar to the taste, and slightly ammoniacal.

Sp. gr. 0.9204.

Boils at about 210° of Fahrenheit: remains perfectly fluid at 32°.

Evaporated at a medium atmospheric temperature, it leaves

about one-sixth of its weight of the peculiar concrete substance, which will be described in the next section: by the assistance of heat, dissolves about one-third its own weight of that substance.

Readily catches fire upon the application of flame, and emits a very great quantity of smoke while burning.

By agitation mixes temporarily with water at the common temperature; from which however it soon separates like oil.

Slightly soluble in boiling water; but in cooling is deposited so as to give a milky appearance to the water, which remains perfectly transparent while at or near the boiling point.

Unites readily with alcohol and with ether at all temperatures.

By agitation with an aqueous solution of potash, or of ammonia, it communicates a slight wheyishness to those fluids; but soon separates from and floats on the top of them.

Absorbs several times its volume of ammoniacal gas, without any sensible change.

Absorbs also several times its volume of muriatic acid gas; becoming, in consequence, opaque and thick.

Forms a uniform white soapy curd with a solution of acetate of lead, by the intervention of an aqueous solution of potash or of ammonia; but, if simply mixed with the metallic solution, it soon separates without any sensible change.

Properties of the white concrete substance.

Taste, pungent and aromatic.

It is particularly characterized by its odour, which is faintly aromatic, and not unlike that of the narcissus and some other fragrant flowers. This odour is readily diffused through the surrounding atmosphere to the distance of several feet, and obstinately adheres for a long time to any substance to which it has been communicated.

When in its purest state, and reduced to powder, it is exceedingly smooth and slightly unctuous to the touch; is perfectly white, and of a silvery lustre.

Sp. gr. rather greater than that of water.

It does not very readily evaporate at the common atmospherical temperature: for, a comparison being made between this substance and camphor, in the quantity of half a grain of each in a very minute state of division, it was found that the camphor had entirely disappeared at the end of 18 hours, while the substance in question had not disappeared entirely at the end of 4 days.

A quantity of it being exposed to heat, in a glass vessel, soon melted; but did not begin to boil till the temperature had reached 410° of FAHRENHEIT: the heat being then withdrawn, it remained liquid till cooled down to 180; at which point the lowest portion was seen suddenly to congeal: the remaining portion congealed gradually; and when the whole had become solid, its temperature was 170°. The structure of the congealed mass was distinctly crystalline, and the crystalline laminæ were slightly flexible.

It is not very readily inflamed; but when inflamed it burns rapidly, and emits an unusually copious and dense smoke, which soon breaks into distinct particles that fall down in every direction.

Does not affect the colour either of litmus or of turmeric.

Insoluble in cold water; and very sparingly soluble in boiling water, from which it separates, in cooling, in such a manner as to render the water milky, which was before transparent: a portion however still remains dissolved, for the water, when filtered, possesses in a slight degree the taste and odour of the substance, and after a few hours deposits it in minute crystals.

Readily soluble in alcohol, and still more so in ether, at any temperature; the solubility, in either instance, greatly increased by increase of temperature.

A solution of this substance in four times its weight of boiling alcohol becomes, in cooling, a solid crystalline mass. It is precipitated from its solution in alcohol by water, without acquiring any additional weight.

It is soluble in olive oil, and in oil of turpentine.

It does not combine either with an aqueous solution of potash or ammonia; nor is it sensibly affected by contact with ammoniacal gas.

Soluble in acetic and in oxalic acid, to each of which it communicates a clear pink colour. A saturated hot acetic solution becomes a solid crystalline mass in cooling.

It blackens sulphuric acid when boiled in it; the addition of water to the mixture having no other effect than to dilute the colour: neither does any precipitation take place upon saturating the acid with ammonia.

Sparingly soluble in hot muriatic acid, to which it communicates a purplish pink colour.

When boiled in nitric acid, it both decomposes the acid, and is itself altered in its composition; and, in cooling, is

abundantly deposited in short acicular crystals aggregated in stelliform groups. These crystals pressed between folds of unsized paper, in order to separate the adhering acid, and then exposed to heat, are readily melted: in cooling, the melted mass shows evident traces of acicular crystallization, and the crystals are of a yellow colour. This yellow substance is readily inflamed, burns with a bright flame, emits much smoke, and leaves a considerable residuum of carbon.

Of all the characters of the white concrete substance described in this section, its ready disposition to crystallize is perhaps the most remarkable.

If thrown into a red hot crucible, a dense white vapour arises from it; which being received into a bell glass placed over the crucible, is condensed round the lower part of the glass in the form of a white powder; but in the upper and cooler part of the glass distinctly crystalline plates are formed, of a beautiful silvery lustre.

A similar and equally beautiful crystallization may be obtained by boiling this substance in water, in a glass matrass having a long neck; in the upper part of which crystals will be formed and deposited during the boiling.

If exposed to a degree of heat not more than sufficient to melt it under a bell glass, the vapour that rises from it crystallizes before it reaches the surface of the glass, and flies about the interior with exactly the appearance of a shower of minute particles of snow.

If a piece of cotton twine be coiled up like the wick of a candle, and after having been dipped in this substance while melted, be set on fire for a second or two, and then blown

out, the vapour will soon begin to crystallize round the wick in very distinct thin transparent laminæ.

This experiment affords one mark of distinction between this substance and benzoic acid, and also between it and camphor: for under similar circumstances, benzoic acid crystallizes in acicular crystals, which are often grouped in a stelliform manner; and camphor crystallizes, or is rather congealed, in globular particles having a stalagmitic appearance.

The most usual crystalline form of this substance is a rhombic plate, of which the greater angle appears to be from 100° to 105°: crystals at least of that form I have repeatedly obtained from its solutions in water, in alcohol, in acetic acid, in the yellow oil described in the last section; and lastly, by melting and very slowly cooling the substance itself. Sometimes several of these plates are variously grouped together; sometimes a single plate intersects another plate at nearly right angles, so that in some points of view the compound crystal appears simply cruciform. The only distinct modifications which I have observed of the common form are a rhomboidal plate, which is very nearly rectangular; and an hexagonal plate: the latter variety may be easily traced from the rhombic plate by the incomplete developement of the smaller angles of the usual rhomb.

The following process has been found most successful in illustrating the crystallization of this substance.

If 25 grains of it be dissolved by the assistance of heat in half a fluid ounce of alcohol, and the solution be cooled slowly in a glass matrass, it will begin to crystallize when

nearly cool; and the matrass being placed between the eye and a tolerably strong light, numerous transparent rhombic crystals will be visible; some of them reflecting from their whole surface a green colour; others, a blue; or a red; or some other of the prismatic colours.

With respect to the elementary constitution of this substance I am not enabled to give any satisfactory information; but it is evident that it contains a very great proportion of carbon. A small quantity of it was passed in the state of vapour through peroxide of copper heated to redness, and the only gaseous product was carbonic acid: whether any water were formed, I could not ascertain.

It cannot be irrelevant to the object of this paper to state, that the white concrete substance which I have been describing, has twice been observed by me in the form of minute crystals, which beautifully reflected the prismatic colours, in the neck of an earthen retort, in which animal matter had been submitted to destructive distillation.

Properties of the yellow farina.

From the minute quantity of this substance which I was capable of obtaining, I could only ascertain one or two of its properties. It is soluble in alcohol, and forms a solution of a bright yellow colour; and it is precipitable from the solution, by the addition of water, in the form of a yellow powder, which remains permanently suspended in the mixture.

When heated, it melts into a substance of the consistence of a soft tough gum of a deep reddish brown colour.

Of the four several substances which result from the distillation of the black liquid described in the former part of this paper, it is probable that the water and the yellow farina are the only real products, and that the others are mere educts of that distillation: for, with respect to the water, its proportion is variable according to the greater or less degree of rapidity with which the distillation is conducted; and if it were present as water in the black liquid, there is reason to believe it would be found supernatant on its surface, after having remained still for some time. The essential liquid oil, and the white concrete substance, which pass over during the distillation, are probably contained originally in that thin portion of the black liquid which may be filtered through unsized paper; for the odour of this filtered portion closely resembles that of the oil; and the oil, by exposure to light, frequently becomes of a darker and darker shade, so as at last to be nearly of a deep brown colour; and, with respect to the white concrete substance, this was not only found crystallized in that part of the original apparatus where the black liquid was condensed, but has been obtained from that liquid by simple evaporation of it at the common temperature of the atmosphere.

The yellow farina is probably produced from the tar which is contained in the proportion of about one-fourth in the black liquid; for it does not make its appearance till towards the end of the distillation; when the more volatile substances have ceased to pass over, and the heat has been increased to the utmost: and if common coal tar be exposed to a low red heat, it will be found, that when the tar has

been nearly evaporated, this yellow farina will begin to pass off.

It remains for me to propose a name for the white concrete substance which has been described in this paper: and, unless a more appropriate term should be suggested by others, I would propose to call it naphthaline.

XVII. *On the aberrations of compound lenses and object-glasses.* *By* J. F. W. Herschel, *Esq. F. R. S. &c.*

Read March 22, 1821.

It has not unfrequently of late been made a subject of reproach to mathematicians who have occupied themselves with the theory of the refracting telescope, that the practical benefit derived from their speculations has been by no means commensurate to the expenditure of analytical skill and labour they have called for, and that from all the abstruse researches of Clairaut, Euler, D'Alembert, and other celebrated geometers, nothing hitherto has resulted beyond a mass of complicated formulæ, which, though confessedly exact in theory, have never yet been made the basis of construction for a single good instrument, and remain therefore totally inapplicable, or at least unapplied, in practice. The simplest considerations, indeed, suffice for the correction of that part of the aberration which arises from the different refrangibility of the differently coloured rays; and accordingly, this part of the mathematical theory of refracting telescopes was soon brought to perfection, and has received no important accession since the original invention of the achromatic object-glass. Indeed the theoretical considerations advanced on this part of the subject by Euler and D'Alembert have even had a tendency to retard its advancement, by appearing to establish relations among the relative

refractive powers of media on rays of different colours which later experimental researches have exploded.

In the more abstruse and difficult part of the theory of optical instruments which relates to the correction of the spherical aberration, the necessity of an appeal to the powers of algebraic investigation has been all along acknowledged; and as the subject is confessedly within its reach, and presents none of those difficulties which obstruct our progress in the transcendental analysis, but merely such as arise from the involved nature of the equations, and the number of symbols which enter into them, it might have been expected that the appeal would, long ere this, have been successful, the artist have bowed to the dictates, however oracular, of a theory which he was satisfied had its foundation in unerring truth, and the result of their combined labours have been the attainment of all the perfection the telescope is susceptible of. Unhappily, however, this is far from being the case. Investigations, it is true, have been accumulated on each other; formulæ have been deduced, and even tables computed from them; but the investigations, from their dry and laborious nature, and the almost total want of that symmetry which is especially necessary in so complicated a subject, have been studied by few; the formulæ, requiring a more extensive share of algebraical knowledge than can be expected in a practical optician, are thrown aside by him in despair, and the tables hitherto constructed from theory, being founded on data which may never again occur, are worse than useless, serving only to mislead. In consequence, the best and most successful artists are content to work their glasses by trial, or by empirical rules, embodying the result of numerous preceding

trials, and which, therefore, have probably some analogy to what would be the final results of theory, if presented in a tangible shape, and accommodated to the peculiarities of their constructions.

The object of the following investigations is to remove or lighten these objections, by presenting first of all, under a general and uniform analysis, the whole theory of the aberrations of spherical surfaces; and in the next place, by furnishing practical results of easy computation to the artist, disentangled from all algebraical complexity, and applicable, by interpolations of the simplest possible kind, to all the ordinary varieties of the materials on which he has to work. In the execution of the former part of this plan, symmetry and simplicity in the disposal of the symbols, is the object chiefly consulted. To attain this, and at the same time avoid circumlocution in the descriptive part of the processes, I have found it necessary to adopt a language somewhat different from that usually employed by optical writers. Instead of speaking of the *focal lengths* of lenses or the *radii* of their surfaces, I speak of their *powers* and *curvatures*, always designating by the former expression, the quotient of unity by the number of parts of any scale which the focal length is equal to; and by the latter, the quotient similarly derived from the radius in question. This mode of expression does no violence to propriety, as the magnifying power of a lens is really inversely proportional to its focal length, and the curvature of a surface is always understood to be reciprocally as its radius; while it gives us the advantage of expressing concisely and naturally, all the most useful propositions in optics. It is certainly simpler (for example) to say, that " the power of any com-

pound lens is the sum of the powers of its separate component lenses," than to express the same thing by saying that "to obtain the focal length of a compound lens, we must divide the product of the focal lengths of its component lenses, by the sum of all the similar products which can be formed by combining them, omitting one in each combination;" or to announce that "the power of a lens is equal to a certain coefficient multiplied by the difference of the curvatures of its surfaces," than to assert that "the focal length is equal to a certain coefficient multiplied by the product of the radii, and divided by their difference." This contraction in language is so convenient, that I hope to see it generally adopted.

The formulæ in the following pages extend no farther than the second term in the developement of the aberration, or that depending on the squares of the semi-apertures. It would have been easy to have carried them to the fourth, and even higher powers; and should object-glasses of *very* great aperture, in comparison with their focal lengths, be ever constructed, it may become necessary; but the dimensions of our present telescopes are far indeed from calling for the immense complexity of algebraic symbols into which this attempt would plunge us; not to speak of the tediousness of the numerical computations, where equations of the tenth and higher degrees are to be resolved. The general value of the aberration for any number of spherical surfaces placed at any finite distances from each other, is assigned by means of an equation of finite differences of the first order. The integration of this presents no difficulty; but I have thought it unnecessary, in the present paper, to pursue it farther in

its developement than was required for its application to the theory of thin lenses placed in contact, and especially to that of double object-glasses, reserving the theory of eye-pieces, microscopes, &c. as well as that of thick lenses, for a second communication, should the Society honour this with a place in their Transactions.

The problem of the destruction of the spherical aberration in a double or multiple lens, is well known to be indeterminate, the algebraic conditions requisite for that purpose furnishing but a single equation (at least when the mean rays only are considered). To fix on the best possible condition for limiting the problem, is a matter of considerable delicacy; D'Alembert has proposed, among others (Opusc. Tom. 3, Art. 742), to annihilate the spherical aberration for rays *of all colours*, a refinement which might almost be termed puerile, were it not for the respect due to so great a name.* It has, besides, the inconvenience of leading to equations of the fourth degree. A much better condition, in every point of view, is another proposed by the same profound geometer, in Art. 758, *viz*: the destruction of the aberration for an object situated out of the axis of the telescope; or in other words, the rendering the whole field of view equally perfect *so far as the object-glass is concerned.* But even this is perhaps carrying refinement too far. The difference of the aberrations of an object-glass in and out of the centre of the field, is so small in ordinary telescopes, as to have escaped (so far as my enquiries have gone), the notice of the best practical opticians

* I pass over the construction proposed by D'Alembert, in Art. 746, as having no recommendation but that of avoiding a biquadratic equation; though, it is true, the radii resulting from it might be used.

(and I have consulted many); nor, of course, has any part of their attention been directed to obviate, experimentally, a source of indistinctness they could not perceive to exist. Clairaut in the Memoirs of the Academy for 1757 has computed the radii of a double object-glass from the condition of their touching throughout the whole extent of their interior surfaces; a very desirable thing in practice, and the curvatures which result are very convenient. Clairaut however has employed in his computations, indices of refraction (1.600 and 1.55) higher, especially the latter, than what are now easily met with; and when the average values, such as are likely to occur most frequently, are employed, the construction becomes imaginary for the more dispersive kinds of glass; and within the limits for which it is real, the radii change so rapidly as to render it difficult to interpolate between their calculated values; so that to the artist who is no algebraist this construction loses much of its real advantage.

There remains a condition unaccountably overlooked (so far as my reading has extended) and which the nature of the formulæ of aberration, as given in the following pages, almost forces on our attention; I mean, the destruction of the aberration not only for parallel rays, or when the telescope is directed to celestial objects, but also for rays diverging from a point at any finite distance. The perfection of the telescope, when directed to land objects, seems to require this; and though, in astronomical telescopes, it may appear uncalled for, the construction possesses other advantages of so high an order as to recommend it even there: these are, 1st. the very moderate curvatures required for the

surfaces: in this respect it has the advantage of most, if not all, of the constructions hitherto proposed on theoretical grounds. 2dly. That in this construction, the curvatures of the two exterior surfaces of the compound lens of given focal length vary within extremely narrow limits by any variation in either the refractive or dispersive powers at all likely to occur in practice. This remarkable circumstance affords a simple practical rule applicable in all ordinary cases, for calculating the curvatures in any proposed state of the data, and requiring only the use of theorems with which every artist must be familiar; and at all events, rendering it extremely easy to interpolate between calculated values. 3dly. That the two interior surfaces approach, in all cases, so nearly to coincidence, that no considerable practical error can arise from neglecting their difference, and figuring them on tools of equal radii. Indeed, for a ratio of the dispersive powers a little above the average, they are rigorously coincident, and this construction coincides with that of CLAIRAUT above-mentioned; and so nearly is this approach to equality of curvature sustained throughout the whole extent of the function, that even when the ratio of the dispersive powers is so low as 0.75 : 1 (a case almost useless to consider) the difference amounts to less than a 40th part of the curvature of each.

§ I. *General formulæ for the focal distances and aberrations of any combination of spherical surfaces.*

1. *Expression for the focal distance of a single spherical surface.*

Let C be the centre of a surface A M (Pl. xix. fig. 1), on which a ray QM proceeding from a point Q in the axis, is incident,

and after refraction let it proceed in the direction Mq. Draw PM perpendicular to QAq, the axis, and put as follows: $\frac{1}{m}=\frac{\text{Sin. Incidence}}{\text{Sin. Refraction}}$ out of the medium QAM into qAM or = the relative refractive index of the medium on which the ray is incident.

y=PM, the semi-aperture

D$=\frac{1}{\text{QA}}$, the reciprocal distance of the radiant point from the surface*

$r=\frac{1}{\text{AC}}$, the reciprocal radius, or curvature of the surface

Put also, for brevity, $\frac{\text{AC}}{\text{AQ}}=\frac{\text{D}}{r}=e$; $\frac{\text{PM}}{\text{MC}}$=Sin. ACM$=s$, and we shall have

1st. $\text{QM}^2=\text{QC}^2+\text{CM}^2-2\text{QC}\,.\,\text{CM}.\cos.\text{ACM}$.

2d. Sin. Incid. = Sin.QMC$=\frac{\text{QC}}{\text{QM}}\cdot s$.

Sin. CMq = Sin. Refrac. $=m\cdot\frac{\text{QC}}{\text{QM}}\cdot s$.

3d. Angle CqM = ACM − CMq.

Sin. CqM=Sin. ACM . cos.CMq − cos.ACM . Sin. CMq.

4th. Cq=CM $\cdot\frac{\text{Sin. CM}q}{\text{Sin. C}q\text{M}}$; A$q$=AC+C$q$.

If we put these expressions into algebraic language, and developing them in powers of s, neglect all beyond the cube of that quantity, we find

$$\text{QM}=\frac{1}{\text{D}}\left\{1+e(1+e)\cdot\frac{s^2}{2}\right\}$$

$$\text{Sin. CM}q=m(1+e)\left\{s-e(1+e)\cdot\frac{s^3}{3}\right\}$$

$$\text{Sin. C}q\text{M}=s\,(1-m-me)+\frac{m(1+e)}{2}\left\{1+e+e^2-m-me\right\}s^3.$$

$$\text{C}q=\frac{1}{r}\cdot\frac{m(1+e)}{1-m-me}\left\{1-\frac{(1-m)(1+e)(m+e+me)}{2(1-m-me)}s^2\right\}$$

* In conformity to the language already explained respecting powers and curvatures, may we not call this the *proximity* of the radiant point?

and finally

$$Aq = \frac{1}{(1-m)r-mer} - \frac{1}{r} \cdot \frac{m(1-m)(1+e)^2(m+e+me)}{2(1-m-me)^2} s^2$$

Let f denote the reciprocal focal distance for central rays, and $f + \Delta f$ the same reciprocal distance for the ray incident at M; then (the aperture being regarded as small in comparison with the focal length), the aberration will be represented by $-\frac{\Delta f}{f^2}$; and if we put for e its value in the foregoing equation, we shall have,

$$f = (1-m)r - m\,\mathrm{D}; \qquad (a)$$

$$\Delta f = m(1-m)(r+\mathrm{D})^2 \left\{mr + (1+m)\mathrm{D}\right\} \times \frac{y^2}{2}; \qquad (b)$$

2. *Theory of the foci of spherical surfaces for central rays.*

Before proceeding to investigate the more complicated cases of the aberrations of several surfaces, we will deduce from the first of these expressions the general equations which determine the place of the focus of central rays after refraction at any number of spherical surfaces; equations we shall have occasion to use hereafter. Let r_1, r_2, r_3, &c. be the curvatures of any number of surfaces A_1, A_2, A_3, &c. which form the common boundaries of the media 0, 1, 2, 3, &c. (fig. 2) and let the relative index of refraction out of the medium 0 into 1 be $\frac{1}{m_1}$, out of 1 into 2, $\frac{1}{m_2}$, and so on; also let μ_0, μ_1, μ_2, &c. be the absolute refractive densities or indices of refraction out of a *vacuum* into these several media, then will

$$\frac{\mu_1}{\mu_0} = \frac{1}{m_1}, \quad \frac{\mu_2}{\mu_0} = \frac{1}{m_1 m_2}, \quad \frac{\mu_3}{\mu_0} = \frac{1}{m_1 m_2 m_3}$$

and so on. Moreover, let t_1, t_2, t_3, &c. be the respective

intervals between the first and second, the second and third surfaces, and so on; or

$$t_1 = A_1 A_2 ; \ t_2 = A_2 A_3 \ \&c.$$

and let f_1, f_2, &c. be the reciprocal distances of the central focus after the 1st, 2d, &c. refraction, from the respective surfaces, or the values of $\frac{1}{A_1 q_1}, \frac{1}{A_2 q_2}$, &c. We here suppose the positive values of r to correspond to surfaces whose convexity is turned towards the original radiant Q (provided its distance be positive) while the positive values of f indicate a situation of the focus q on the opposite side of the surface. With regard to t, its values are necessarily positive in cases of refraction, but when $m = -1$, which corresponds to those of reflexion, (which are thus equally included in the present analysis,) t has a negative value.

This premised, if we make $D = o$, or the distance of the radiant point infinite, the focus for parallel and central rays will be assigned by the equation

$$f = (1 - m) \cdot r$$

Let φ denote this value of f, and $\varphi_1 = (1 - m_1) r_1$, &c.: then will φ_1, φ_2, &c. denote the reciprocals of the *principal* focal lengths of the several surfaces, *in situ*, i. e. supposing the adjacent media in each case continued to infinity. We have then in general the equation

$$f = \varphi - mD.$$

Suppose now f and f', m and m', φ and φ' to represent any two consecutive values of f, m, and φ in the series f_1, f_2, f_3, &c. &c. Then, since the focus after any refraction becomes the radiant point corresponding to the next, we have

$$\frac{1}{D'} = -\left(\frac{1}{f} - t\right) = -\frac{1-ft}{f}$$

and the equation $f' = \varphi' - m' D'$ becomes

$$f' = \varphi' + \frac{m'f}{1-ft}. \qquad (c)$$

This is in fact an equation of differences between the consecutive values of f, and the general value may therefore be obtained by integration, or its particular ones deduced in succession from each other, when the integration is impracticable from the values assigned to m, φ, and t. The greatest simplification it appears to admit, is its reduction to an equation of the 2d order and first degree, which may be performed by assuming

$$f = \frac{u'}{u} + \frac{1}{t}$$

when the equation will become, after the necessary reductions,

$$o = u'' - \left(\varphi' - \frac{1}{t'} - \frac{m'}{t'}\right) u' + \frac{m'}{t^2} u$$

It will not be necessary to examine particularly all the integrable cases, or to discuss at present the form of the general value of u or f in terms of φ, m, and t. This latter subject is elegantly treated by LAGRANGE, in a Memoir "Sur la Theorie des Lunettes," in the collection of the Academy of Berlin, (Acad. Berl. 1778), to which we may refer. We need only remark that, whatever be its integral, it must necessarily be of the form

$$\frac{\text{M}-\text{ND}}{\text{O}-\text{PD}}$$

The original distance of the first radiant entering as the arbitrary constant, and being therefore always involved in the same simple algebraic form, whatever be the number and position of the surfaces.

3. Two cases of the equation, however, are worthy of a more particular examination. The first is, when the number of surfaces is infinite, and the intervals separating them infinitely

diminished. In this case the refractive power of the medium varies by insensible gradations; and if we suppose both it and the radii of curvature of the layers of equal density to vary according to a given law, we shall have both μ and r, expressed in functions of the depth to which the ray has penetrated at any moment of its course. This is the case with the crystalline lens of the eye. Dr. BREWSTER's observations have demonstrated that this humour increases very rapidly in density from the circumference to the centre; and to apply our general equation to the evaluation of its focal length, we must proceed as follows. Taking x to represent the depth of any layer whose thickness in the middle is dx, the curvatures r and r' of its surfaces will be r and $r+dr$. We have also, $t=dx$, $t'=dx+d^2x$, taking x for the independent variable.

Moreover, since $\frac{\mu}{\mu'}=m'$, we have

$$m'=\frac{\mu}{\mu+d\mu}=1-\frac{d\mu}{\mu}$$

and in consequence, $1-m'=\frac{d\mu}{\mu}$. Hence we obtain

$$\varphi'=(1-m')\,r'=\frac{rd\mu}{\mu}$$

neglecting the products of the infinitely small quantities, so that our equation (c) becomes (since $f'=f+df$)

$$f+df=\frac{rd\mu}{\mu}+\frac{\left(1-\frac{d\mu}{\mu}\right)\cdot f}{1-fdx}$$

which developed, retaining only terms of the first degree, gives

$$f+df=f+f^2\,dx-f\frac{d\mu}{\mu}+\frac{rdm}{\mu}$$

or simply, putting $\mu=e^v$ where $e=2{\cdot}7182818$, &c.

$$o=df+(f-r)\,dv-f^2\,dx. \qquad (d)$$

The integration of this equation, in which r and ν are given functions of x, must be performed on the hypothesis that when $x=o$, $f=\left(1-\frac{1}{m}\right)r-\frac{D}{\mu}$, where μ and r have their initial values. Dr. YOUNG has given a solution of a particular case of this difficult problem in his paper on the Mechanism of the Eye, in the Phil. Trans. for 1801, p. 32.

4. The other case of our equation (c) which we proceed to examine, is that, where the surfaces are finite in number, and placed close together, so as to form a compound lens infinitely thin in the middle. In this case we have $t=o$, and the equation becomes simply

$$f'=\varphi'+m'f$$

which (putting $f_0=-D$, $\mu_1=\frac{1}{m_1}$, $\mu_2=\frac{1}{m_1 m_2}$), &c. gives at once by integration

$$f\,(\text{or } f_n)=\frac{1}{\mu_n}\left\{\mu_1\,\varphi_1+\mu_2\,\varphi_2+\ldots\ldots\mu_n\,\varphi_n-D\right\}; \qquad (e)$$

If, after passing out of a vacuum through any number n of surfaces, the ray emerge again into a vacuum, we have $\mu_n=1$, and

$$f=\mu_1\,\varphi_1+\mu_2\,\varphi_2\,\ldots\ldots+\mu_n\,\varphi_n-D; \qquad (e')$$

If in this equation we put for φ_1, φ_2, &c. their values in terms of r_1, r_2, &c., and put

$$\mu_1(1-m_1)=k_1,\ \mu_2(1-m_2)=k_2,\ \&c.$$

we shall obtain

$$f=\frac{1}{\mu_n}\left\{k_1\,r_1+k_2\,r_2+\ldots\ldots k_n\,r_n-D\right\}; \qquad (f)$$

Suppose now the radiant point to be infinitely distant, or $D=o$, then will f become the *principal* reciprocal focal length or power of the system; and calling this F, we get

$$F = \frac{1}{\mu_n}\left\{ k_1 r_1 + k_2 r_2 + \ldots . k_n r_n \right\}; \qquad (f')$$

$$f = F - \frac{D}{\mu_n} \qquad (f'')$$

or, when the last refraction is made into a *vacuum*,

$$f = F - D \qquad (f''')$$

5. Let us imagine a system of *n lenses*, each consisting of a single medium, placed close together in *vacuo*, and as the ray after traversing each separate lens emerges into a *vacuum*, we have $\mu_2 = \mu_4 = \ldots .$ &c. $= 1$; $m_1 m_2 = 1, m_3 m_4 = 1$, &c. and therefore $k_1 = \mu_1 - 1 = -k_2, k_3 = \mu_3 - 1 = -k_4$, &c. so that our expression for F becomes

$$F = k_1 (r_1 - r_2) + k_3 (r_3 - r_4) + \text{\&c.}$$

or, denoting μ_1, μ_3, &c. simply by μ, μ', &c.

$$F = (\mu - 1)(r_1 - r_2) + (\mu' - 1)(r_3 - r_4) + \text{\&c.} \qquad (g)$$

In the case of a single lens, this reduces itself to its first term, and calling L, L′, &c. the powers of the several lenses, we have

$$L = (\mu - 1)(r_1 - r_2); \; L' = (\mu' - 1)(r_3 - r_4); \; \text{\&c.} \qquad (h)$$

and finally

$$F = L + L' + L'' + \text{\&c.} \qquad (i)$$

which expresses that *the power of a system of lenses* (placed close together and infinitely thin), *is the sum of the powers of its component lenses.* The powers of concave lenses are here regarded as negative, as well as their focal lengths; while the equation (f''') shows that the sum of the reciprocal distances of the object and its image is equal to the power, or reciprocal focal length, of the system.

6. These propositions are sufficiently well known, and comprise the whole theory of the central foci of infinitely

thin lenses in contact. Let us next examine how these results will be modified by taking into consideration a small, but finite thickness in each of the lenses. To this end we may proceed as follows:

If $U=o$ be any equation of differences involving f, f', and t, where t and its values are so small as to permit their powers and products to be neglected; suppose (f) to be the value of f deduced from the equation on the supposition that $t=o$, then in general we may take

$$f=(f)+u$$

where u is a quantity of the same order with t. If this be put for f in $U=o$, the equation, by developing, and rejecting the powers and products of u and t, will take the form

$$o=V+W.u+X.u'+Y.t$$

V, W, X and Y being functions of (f) and (f'), and it is evident that V vanishes by reason of the values assigned to these quantities. There remains then a linear equation of the first order between u and u', which is easily integrated. In the case before us, we have

$$u=f'-\varphi'-\frac{m'f}{1-ft}=o$$

which developed becomes

$$f'=\varphi'+m'f+m'f^2t$$

In this, writing $(f)+u$ for f, and retaining only the terms multiplied by u', u, and t, we get

$$u'-m'u=m'\,(f)^2\,t$$

and integrating

$$u=m_2 m_3 \ldots m_n (f_1)^2 t_1 + m_3 \ldots m_n (f_2)^2 t_2 + \ldots$$
$$\ldots\ldots + m_n (f_{n-1})^2 t_{n-1}.$$

Hence it is easy to conclude, that if we have *n lenses* placed

in contact, whose thicknesses are respectively t, t', t'', &c., and powers (neglecting their thicknesses) L, L′, L″, &c. their refractive densities being respectively μ, μ', μ'', &c. and the curvatures of their *anterior* surfaces, r, r', r'', &c. then will the reciprocal distance of the image from the posterior surface of the last lens be given by the equation.

$$\begin{aligned} f = {} & \mathrm{L} + \mathrm{L}' + \mathrm{L}'' + \&\text{c.} - \mathrm{D} \\ & + m \left\{ (\mu - 1)\, r - \mathrm{D} \right\}^2 t \\ & + m' \left\{ \mathrm{L} + (\mu' - 1)\, r' - \mathrm{D} \right\}^2 t' \\ & + m'' \left\{ \mathrm{L} + \mathrm{L}' + (\mu'' - 1)\, r'' - \mathrm{D} \right\}^2 t'' + \&\text{c.} \ldots\ldots \quad (j) \end{aligned}$$

continued to as many terms as there are lenses. In this equation m, m', m'', &c. are the reciprocals of μ, μ', μ'', &c.

General theory of the aberrations of spherical surfaces for rays incident in the plane of the axis.

7. Let us next proceed to investigate the spherical aberration of any system of surfaces. Suppose the ray, after passing through the n^{th} surface, to be incident on the $(n+1)^{th}$: its aberration here will arise from two causes; 1st. that after traversing the n preceding surfaces, instead of converging to, or diverging from the focus for central rays, its direction was really to or from a point in the axis, distant from that focus by the total aberration of those n surfaces; and, 2dly that, being incident at a distance from the vertex of the $(n+1)$th surface, a new aberration will be produced here, which (being, as well as the other, of small amount) the principles of the differential calculus allow us to regard as independent of it, and which, being computed separately and added to it, gives the whole aberration of the system of $n+1$ surfaces.

The same is true of the small alterations in the values of f produced by the aberrations. If we denote by δf the change in the value of f due to the action of the n preceding surfaces by $\delta' f$, that due to the action of the $(n+1)$th, and by Δf the total change arising from their combined action, we shall have

$$\Delta f = \delta f + \delta' f$$

Now, 1st. to investigate the partial alteration $\delta f'$ in the value of f' arising from the total alteration Δf in that of f, we resume our equation (c); and differentiating its first member according to the characteristic δ and its second according to Δ, we get (f and f' being the only variables)

$$\delta f' = \frac{m' \Delta f}{(1-ft)^2}.$$

2dly. to discover the partial variation $\delta' f'$ of f', arising immediately from the action of the $(n+1)$th surface, we have, by the equation (b) writing $\delta' f'$ for Δf, and m', r', D', y' respectively for m, r, D and y,

$$\delta' f' = \frac{y'^2}{2} \cdot m' (m'-1) (r'+D')^2 \left\{ m' r' + (1+m') D' \right\}$$

but, in this case, neglecting the fourth and higher powers of the semi-apertures, it is easy to see that

$$y' = y (1-ft)$$

and substituting this for y' and $-\frac{f}{1-ft}$ for D', the equation becomes,

$$\delta' f' = \frac{y^2}{2} \cdot m' (1-m') (r'-f-fr't)^2 \left\{ m' r' - \frac{(1+m')f}{1-ft} \right\};$$

so that, uniting the two variations, we find

$$\Delta f' - \frac{m'}{(1-ft)^2} \Delta f =$$

$$= \frac{y^2}{2} \cdot m' (1-m') (r'-f-fr't)^2 \cdot \left\{ m' r' - \frac{(1+m')f}{1-ft} \right\}. \qquad (k)$$

If the surfaces be placed close together, or $t=o$, this becomes

$$\Delta f' - m' \Delta f = \frac{y^2}{2} \cdot m' (1-m') (r'-f)^2 \left\{ m' r' - (1+m') f \right\} \ldots\ldots (l)$$

but when this is not the case, perhaps it will be found more convenient to use one of the following equations

$$\Delta f' - \frac{m'}{(1-ft)^2}\Delta f =$$

$$= \frac{y^2}{2}(1-m')f^2 \cdot \left\{\frac{r'-f'}{f'-\varphi'}\right\}^2 (r'-(1+m')f'); \qquad (m)$$

or,

$$\Delta f' - \frac{m'}{(1-ft)^2}\Delta f =$$

$$= \frac{y^2}{2}(1-m')\left\{r'-f-fr't\right\}^2 . (r'-(1+m')f'); \qquad (m')$$

which are derived from it by eliminating t, either wholly or partially from the second member, by the help of equation (c). These equations are universally integrable, and suffice to assign the aberration in any proposed combination of spherical surfaces, however placed.

Theory of the aberrations of infinitely thin lenses placed in contact.

8. Confining ourselves at present to this branch of our subject, it will easily appear on integrating equation (l) that

$$\Delta f = \frac{1}{\mu_n}\left\{\mu_1 Q_1 + \mu_2 Q_2 + \dots \mu_n Q_n\right\}$$

where we have

$$Q_n = \frac{y^2}{2} m_n (1-m_n)(r-f_{n-1})^2 \left\{m_n r_n - (1+m_n) f_{n-1}\right\}$$

in which it will be recollected that the value of f_0 is $-$ D.

Let us now examine the composition of this function more particularly; and first, supposing $n=2$, the case of a single lens placed in *vacuo*, we have $\mu_2 = 1$, $m_2 = \frac{1}{m_1} = \mu_1$ and

$$\Delta f = \mu_1 Q_1 + Q_2$$

If then we write μ and m for μ_1, and m_1, and make all reductions, we get

$$Q_1 = \frac{m(1-m)}{2} y^2 \left\{ m r_1^3 + (1+3m) r_1^2 D + (2+3m) r_1 D^2 + (1+m) D^3 \right\}$$

$$Q_2 = \frac{1-m}{2m^3} y^2 \left\{ \begin{array}{l} r_2^3 + (m^2+2m-3) r_2^2 r_1 + (2m^3 - m^2 - 4m + 3) r_2 r_1^2 - (1+m)(1-m)^3 r_1^3 \\ + \left\{ (3m+m^2) r_2^2 + (4m^3 + 2m^2 - 6m) r_2 r_1 + 3m(1+m)(1-m)^2 r_1^2 \right\} D \\ + \left\{ (2m^3 + 3m^2) r_2 - 3m^2 (1+m)(1-m) r_1 \right\} D^2 + m^3 (1+m) D^3. \end{array} \right.$$

The expression of $\mu Q_1 + Q_2$ in terms of r_1 and r_2 will be simplified, if we recollect that when $r_1 = r_2$ the lens will have no aberration, being in this case merely an infinitely thin spherical lamina, equally thick in all parts, through which all rays pass without deviation. $\triangle f$ must consequently vanish when $r_1 = r_2$, and must therefore have $r_1 - r_2$ for a factor; so that $\mu Q_1 + Q_2$ must be divisible by this without remainder. Observing this, we get, on making the reductions,

$$\triangle f = \mu^2 (\mu - 1) \frac{y^2}{2} (r_1 - r_2) \left\{ \begin{array}{l} \alpha r_1^2 + \alpha' r_1 r_2 + \alpha'' r_2^2 \\ + (\beta r_1 + \beta' r_2) D \\ + \gamma D^2 \end{array} \right\}; \quad (n)$$

provided we take

$$\alpha = 2m^3 - 2m + 1; \quad \beta = 4m^3 + 3m^2 - 3m; \quad \gamma = 2m^3 + 3m^2$$
$$\alpha' = m^2 + 2m - 2 \quad \beta' = m^2 + 3m$$
$$\alpha'' = 1$$

or, if we make

$$A = \alpha r_1^2 + \alpha' r_1 r_2 + \alpha'' r_2^2; \quad B = \beta r_1 + \beta' r_2; \quad C = \gamma,$$

then

$$\triangle f = \mu^2 (\mu - 1) \frac{y^2}{2} (r_1 - r_2) \left\{ A + B D + C D^2 \right\}$$

Now, it has been shown that, L being the power of the lens,

$$L = (\mu - 1)(r_1 - r_2)$$

consequently the above value of $\triangle f$ reduces itself to

$$\triangle f = \frac{y^2}{2} \times \mu^2 L \left\{ A + B D + C D^2 \right\} \cdot \quad (n')$$

9. Suppose now we place any number of lenses close together in *vacuo*, then we shall have, as in Art. 5,

$$\mu_2 = \mu_4 = \&c. = 1\,;\ m_1 m_2 = 1\,;\ m_3 m_4 = 1,\ \&c.$$

But we have also

$$f_3 = \frac{1}{\mu_3}\left\{k_1 r_1 + k_2 r_2 + k_3 r_3 - \text{D}\right\}$$
$$= \frac{1}{\mu_3}\left\{k_3 r_3 + f_2\right\}$$

whence we see that f_3 is formed from $-f_2$ precisely *(mutatis mutandis)* as f_1 is from D; and it is therefore evident that if we take L′, A′, B′, C′, the same functions of the refractive index and radii of the second lens, that L, A, B, C, are of those of the first; and put $\text{D}' = -f_2 = -(\text{L} - \text{D})$ and write μ' for μ_3, the refractive index of the second lens, we must have

$$\mu_3 \text{Q}_3 + \mu_4 \text{Q}_4 = \frac{y^2}{2} \times \mu'^2 \text{L}'\left\{\text{A}' + \text{B}'\text{D}' + \text{C}'\text{D}'^2\right\}$$

and similarly for the value of $\mu_5 \text{Q}_5 + \mu_6 \text{Q}_6$ adding another accent to the letters in the second member, and observing that $\mu'' = \mu_5$ and

$$\text{D}'' = -(\text{L}' - \text{D}') = -(\text{L} + \text{L}' - \text{D})$$

and so on, so that we have ultimately, whatever be the number of lenses,

$$\Delta f = \frac{y^2}{2}\left\{\mu^2\text{L}(\text{A}+\text{BD}+\text{CD}^2) + \mu'^2\text{L}'(\text{A}'+\text{B}'\text{D}'+\text{C}'\text{D}'^2) + \&c.\right\}; \quad (o)$$

continued to as many terms as there are lenses.

If the surfaces of a compound lens be in optical contact, (*i. e.* if the media of which it consists, instead of having thin lenses of air or *vacuum* interposed, be contiguous, the convexity of one fitting exactly into the concavity of the other, as in the case of two glass lenses inclosing a fluid), we may still regard them as separated by infinitely thin, non-refractive laminæ, having equal curvatures on both sides, for it is obvious that these will produce no deviation. In this case,

if the curvatures of the proposed system of surfaces be r_1, r_2, r_3, &c., those of the equivalent system will be r_1 and r_2, r_2 and r_3, &c. In this case we have

$$L = (\mu - 1)(r_1 - r_2)\,;\ L' = (\mu' - 1)(r_2 - r_3)\ \&c.$$

and taking these as the values of L, L′, &c. the equation *(o)* will still hold good.

Of the mode of correcting the aberrations of a compound lens; and first, of the destruction of the spherical aberration in two lenses of the same medium placed close together, with a view to the improvement of magnifiers, eye-glasses, and burning lenses.

10. The value of $\triangle f$ in a single lens, for parallel rays, is represented by $\frac{y^2}{2} . \mu^2 L A$. If we put for A its value, and attempt to make this vanish by assigning a relation between r_1 and r_2, we shall find the roots of the equation imaginary, unless the refractive index exceed 4, a case which nature affords no approach to. If we would reduce it to its minimum value, we find

$$\frac{r_2}{r_1} = \frac{2 - m - 4m^2}{2 + m}.$$

In ordinary glass, we have $\mu = 1.524$, or nearly $\frac{3}{2}$ whence we get $m = \frac{2}{3}$, $\alpha = +\frac{7}{27}$, $\alpha' = -\frac{6}{27}$, $\alpha'' = 1$, $\beta = +\frac{32}{27}$, $\beta' = +\frac{66}{27}$, $\gamma = +\frac{52}{27}$. In such glass therefore we have $\frac{r_2}{r_1} = -\frac{1}{6}$,* or the lens must be double convex or double concave, having the radius of the posterior surface six times that of the anterior.

* In strictness 0.1466, or little more than $\frac{1}{7}$; but this part of the subject being of less moment, I have used $\frac{3}{2}$ for the value of μ, to facilitate the calculations.

The aberration of such a lens being computed (for a given power L) will be found to equal $-\frac{15}{14}y^2$ L. Let this be called ω and we shall have the proportional aberrations of the following lenses as below:

Plano-convex or concave, plane side first .	$4 \cdot 2 \times \omega$
Do. curved surface first . .	$1 \cdot 081 \times \omega$
Double-equi-concave or convex . . .	$1 \cdot 567 \times \omega$

The aberration, for parallel rays, of a double lens, of which the first glass has a and b for the curvatures of its surfaces, and the second (of the same substance) a' and b', is represented by

$$-\frac{y^2}{24(L+L')^2}\left\{\begin{array}{l} L(7a^2-6ab+27b^2) \\ +L'(7a'^2-6a'b'+27b'^2-(32a'+66b')L+52L^2)\end{array}\right\}$$

In this if we suppose $a=h\,.\,2\,L$, $a'=h'.\,2\,L'$, which give $b=(h-1)\,.\,2\,L$, $b'=(h'-1)\,.\,2\,L'$

$$\frac{b}{a}=\frac{h-1}{h},\ h=\frac{a}{a-b},\ \&c.$$

and suppose moreover, $x=\frac{L'}{L}$, and

$$X=(28\,h'^2-48\,h'+27)\,x^3+(33-49\,h')\,x^2$$
$$+13\,x+(28\,h^2-48\,h+27),$$

we shall have, for the expression of the aberration of the compound lens,

$$-\frac{y^2(L+L')}{6}\cdot\frac{X}{(1+x)^3}=\Omega\,.$$

The aberration of the best single lens of equal power is, as we have already found, $-\frac{15}{14}y^2(L+L')$, and comparing the two, we have

$$\frac{\Omega}{\omega}=\frac{7}{45}\cdot\frac{X}{(1+x)^3}\,; \qquad (p)$$

11. If we would destroy the aberration, we have only to put $X=o$. As this cubic equation must have at least one

real root, it follows that whatever be the proportion of the curvatures of the surfaces of two thin lenses placed close together, it is always possible to adjust their focal lengths so as to produce a combination free from spherical aberration; and the same is true if the lenses be formed of different materials.

To take an example or two; suppose the first and third surfaces on which the light falls to be plane, and we have $a = a' = o$, and consequently $h = h' = o$, so that the equation $X = o$ becomes

$$27x^3 + 33x^2 + 13x + 27 = o$$

whose only real root is $x = -1{\cdot}392$. Hence, if we take $L = \pm 1$, we have $L + L' = \mp 0{\cdot}392$. So that the power of the compound lens is about $\frac{2}{5}$ that of the first glass and of an opposite nature, which, though moderate, may not be too low to be of some use.

If an object be placed in the focus of parallel rays so formed, the rays it sends to every part of the surface will emerge rigorously parallel. Such an object will therefore be seen by an eye on the other side with as much distinctness as if it were a real object at an infinite distance, subtending the same angle, and the combination may thus be used as an eye-glass or magnifier, as well as an object-glass, only reversing its position with respect to the eye.

12. Let $a = b' = o$, $h = o$, $h' = 1$, or suppose the first and last surfaces plane, and our equation $X = o$ becomes

$$7x^3 - 16x^2 + 13x + 27 = o$$

which has but one real root, $x = -0{\cdot}8517$, or

$$L' = -L \cdot 0{\cdot}8517\,;\ L + L' = L \times 0{\cdot}1483$$

The lenses then must be of opposite characters, but the

power of the compound being only about $\frac{1}{7}$ of that of the first lens, is too low to be of service.

On the same hypothesis as to the plane surfaces, if we trace the variation of the function $\frac{X}{(1+x)^3}$, we shall find that it admits a minimum for a positive value of x given by the equation

$$37x^2 - 58x - 68 = o$$

viz. $x = 2{\cdot}349$. This gives

$$L' = 2{\cdot}35 \times L\,;\ L + L' = 3{\cdot}35 \times L$$

and,
$$\frac{\Omega}{\omega} = 0{\cdot}24841$$

This is the minimum value which the ratio of aberrations admits for a positive value of x. The combination is represented in Pl. XIX. fig. 3; and we see that a very material superiority over the best single lens of the same power is the result of such a disposition, the aberration being reduced to less than a fourth part. Even if the plano-convex lenses thus laid together be of equal focus, the value of $\frac{\Omega}{\omega}$ will be only $0{\cdot}6028$, indicating still a sensible advantage gained over any single lens.

13. Let us however take up the problem more generally, and enquire what should be the curvatures of all the four surfaces to destroy the whole aberration in the most advantageous manner with respect to the power of the resulting combination. To this end it is evident, that (the equation $X = o$ still subsisting) we must also have $L + L =$ a maximum, and since we may assume as given the power of the first lens (without which the problem is indefinite), we have $dL = o$ and $dL' = o$, whence, $dx = o$ also, differentiating then the equation $X = o$, on this hypothesis, we have

$$(56h-48)dh+\left\{(56h'-48)\,x-49\right\}x^2dh'=o.$$

the independent parts of which being made to vanish separately, we find,

$$h=\frac{6}{7},\ h'=\frac{6}{7}+\frac{7}{8x}$$

The former of these determines at once the form of the anterior glass, which must be a double convex or concave of the best form, (the curvatures as 6 : 1) in its best position. The latter being substituted in X$=o$ gives

$$x^3-1\cdot4\times x^2-1\cdot3125\times x+1=o.$$

all whose roots are real, viz:

$$x=-0\cdot9798,\ x=+0\cdot5609,\ x=+1\cdot8193$$

The first of these values gives the worst possible mode of correcting the aberration, the second lens almost exactly neutralizing the first. The second destroys the aberration by the application of a correcting lens whose effect in altering the power is the smallest, while the third is that which affords the greatest possible power. If we execute the numerical computations in the two latter cases we shall find the dimensions as follows:

	2d Case.	3d Case.
Focal length of the 1st lens	+10·000	+10·000
Radius of its 1st surface -	+ 5·833	+ 5·833
———— 2d ——— -	—35·000	—35·000
Focal length of the 2d lens -	+17·829	+ 5·497
Radius of its 1st surface -	+ 3·688	+ 2·054
———— 2d ——— -	+ 6·291	+ 8·128
Focal length of the combination	+ 6.407	+ 3·474

These combinations are represented in Pl. XIX. figs. 4 and 5.

Whether we ought or not to aim at the rigorous destruction of the aberration of rays parallel to the axis, the use

to which the lens is to be applied must decide. In a burning glass it is of the highest importance. A slight consideration will suffice to show, that the difference of temperatures produced in the foci of a double convex lens of equal radii, and one of the same focal length but of the best form, must be very considerable. In order to try whether even the latter might not be improved by the shortening of the focus, and the superior concentration of the exterior rays, by applying a correcting lens of one of the forms above calculated, in spite of the loss of heat in passing through a second glass, I procured two lenses to be figured to the radii assigned in the first column of the foregoing table. They were about three inches in aperture, and when combined as above directed, the aberration was almost totally destroyed, and probably would have been so completely, had the index of refraction proper to the glass been employed, instead of that adopted in our calculation for brevity. Their combined effect as a burning lens appeared to me decidedly superior to that of the first lens used alone, and there is therefore good reason to presume that the effect of the other construction which, with the same loss of heat, affords a much greater contraction of the focus would be still better, and I regret not having tried it in preference.

14. In eye-glasses and magnifiers, if we would examine a minute object with much attention, as a small insect, or (when applied to astronomical purposes) if we would scrutinize the appearance of a planet, a lunar mountain, the nucleus of a comet, or a close double star, where extent of field is of less consequence than perfect distinctness in the central point, too much pains cannot be taken in destroying the central aberra-

tion. There is another case in which an aplanatic eye-glass should be employed, viz. in examining the parabolic figure of a speculum, or the perfect adjustment of an object-glass. If the surface of the speculum or object-glass be divided into concentric annuli by diaphragms, covering different parts of it in succession, the rays incident on these, after crossing in their focus will be spread over corresponding annuli on the surface of the eye-glass, and if the distance between the mirror and eye-glass when adjusted to perfect vision, continue the same for all the annuli, we conclude that the figure of the speculum is perfect. It is so however only with respect to that particular eye-glass; and if the aberrations of this be not corrected, all the pains of the artist will only produce a mirror affected with proportional and opposite imperfections. It is true, the use of a very high magnifying power obviates this objection in great measure, by confining the aberration of the eye-glass within a narrower compass; but it is better in theory, and undoubtedly more convenient in practice, to annihilate it altogether. The aberration in the eye appears to me to be entirely out of the question here, but the consideration of that point would lead us away from the present subject.

On the other hand, when a moderately distinct, but extensive field of view is of more consequence than a perfect, but confined one, as in spectacles, reading glasses, magnifiers of moderate power, and eye-glasses for certain astronomical purposes, the correction of the aberration in the centre of the field, may be sacrificed with little inconvenience. By far the best periscopic combination I am acquainted with, consists of a double convex lens of the best form, but placed in its worst

position, for the lens next the eye, and a plano-concave whose focal length is to that of the other as 2·6 : 1 or as 13 : 5, placed in contact with its flatter surface and having its concavity towards the object, as in Pl. XIX. fig. 6, for the farthest: yet for destroying the aberration of rays parallel to the axis, nothing can be worse. In fact our formula (p) gives for the aberration in this construction

$$\frac{\Omega}{\omega} = 22{\cdot}302$$

or about 22 times what the best single lens of equal power would give: yet on accidentally combining two such lenses in this manner, I was immediately struck with the remarkable extent of oblique vision,* with the absence of fatigue, on reading some time with a power much beyond that of the natural eye, and with the freedom from colour at the edges of the field, arising from *the opposition of the prismatic refractions* of the two solids, an advantage which a single meniscus does not possess.

Theory of object-glasses; and first, of the destruction of the chromatic aberration, or the imperfections arising from the different refrangibility of the rays of light.

15 The perfection of an object-glass requires that parallel or diverging rays of all colours incident on every point of the glass, should converge to one and the same point, and consequently, that we should have f invariable and $\triangle f$ zero, for all the colours of the spectrum. With regard to the latter

* The focal length of the compound lens tried, was 1·84 inch; the field of tolerably distinct vision extended full 40° from the axis, and the forms of objects were distinguishable (the letters of a book might be read) with management, as far as the 75th degree. The lenses used in this combination should be very thin, and the eye applied as close as possible.

condition, we may content ourselves as already remarked, with annihilating Δf for the most luminous rays, but the former must be satisfied as rigorously as possible. To fix the colour of a ray, we may either fix its position in a spectrum cast by a prism of a standard substance, or the length of its fits of easy transmission and reflexion in *vacuo*. The latter method is on all accounts preferable. The whole difference then between the lengths of the fits of an extreme red and violet ray being taken for unity, let c be difference between those of a ray of any assumed colour and those of the most luminous ray in the spectrum, c being positive for rays nearer the red end of the spectrum, and negative for those nearer the violet. Then in different media, the refractive indices μ, μ', &c. for that colour will be functions of c of a form depending on the nature of the media, and which perhaps is not the same for any two media in nature. What this form is in any one medium is at present altogether unknown, but in all, we may represent it by

$$\mu + (\mu - 1)\left\{pc + qc^2 + rc^3 + \&c.\right\}$$

p being the quantity usually termed the dispersive power of the medium, and which even in the most dispersive bodies hitherto observed does not exceed 0·4, and is generally a very small fraction, while q, r, &c. are numerical co-efficients, whose influence was perceived shortly after the discovery of the different dispersive powers of bodies, by Clairaut, and whose real existence the experiments of Blair, Brewster, &c. seem to have placed beyond a doubt. The presumption is that they decrease rapidly in magnitude, and are altogether insensible in the higher terms of the series.

Let this assumed value of the refractive index be put for μ in our equation

$$f = (\mu-1)(r_1-r_2) + (\mu'-1)(r_3-r_4) + \&c. - D$$

and similar values for μ', μ'', &c., and we get

$$\begin{aligned} f = & (\mu-1)(r_1-r_2) + (\mu'-1)(r_3-r_4) + \&c. - D \\ & + c\left\{p(\mu-1)(r_1-r_2) + p'(\mu'-1)(r_3-r_4) + \&c.\right\} \\ & + c^2\left\{q(\mu-1)(r_1-r_2) + q'(\mu'-1)(r_3-r_4) + \&c.\right\} \\ & + \&c. \end{aligned}$$

or simply (L, L′, &c. designating the powers of the several lenses for the most luminous rays.)*

$$\begin{aligned} f = & L + L' + L'' + \&c. - D; \qquad (q) \\ & + c\left\{Lp + L'p' + L''p'' + \&c.\right\} \\ & + c^2\left\{Lq + L'q' + L''q'' + \&c.\right\} \\ & + \&c. \end{aligned}$$

In order then that this may remain the same for rays of all colours, it must be verified independent of any particular value assigned to c, a condition which gives

$$\left.\begin{aligned} o &= Lp + L'p' + L''p'' + \&c. \\ o &= Lq + L'q' + L''q'' + \&c. \\ &\&c. \end{aligned}\right\}; \qquad (r)$$

These equations, being infinite in number, while the number of the quantities L, L′, &c. is limited, it is of course impossible to satisfy them all, by any adaptation of the latter quantities; but as q, r, &c. decrease rapidly, we may confine ourselves to satisfy one or two of the first, as the others can produce but

* It were to be wished that in physical optics the *most luminous rays* were always employed as the term of comparison. The mean or middle ray of the spectrum varies in every different medium, and has no distinguishing property which renders it susceptible of exact determination, while the others, by their presence or absence, uniformly mark the maxima and minima of optical phænomena.

an insensible change in the value of f, especially since the greatest value of c very little exceeds $\frac{1}{2}$.

Opticians usually regard only the co-efficients p, p', &c. which represent the dispersive powers; and the first of our equations (r), which assigns a relation among the powers of the lenses of a very simple nature, has in general been the only one resorted to to insure the achromaticity of the system. It has long however been a subject of complaint, that however perfectly the foci of a double object-glass be adjusted to unite the extreme rays of the spectrum, a more or less considerable quantity of uncorrected colour remains, which cannot be destroyed by such adjustment. This is obviously owing to the non-proportionality of the quantities of p, q, r, in different media, which renders it impossible to satisfy more than the first of the equations (r), or to what is termed the irrationality of the coloured spaces, in the spectra; and the attention of the optical philosopher has for some time past been turned to the discovery of media, in which either this defect of proportionality shall be imperceptible, or else so considerable, as to admit a more perfect correction by the use of three lenses of different media, so adjusted as to satisfy two of the equations. As the co-efficients q, r, &c. furnish equations exactly similar to those afforded by p, it would not be amiss, were they designated in future by the epithet of dispersive powers of the 2d, 3d, and superior orders, and were each medium regarded as having its own peculiar system of dispersive powers of all orders to infinity according to the values of p, q, r, &c. It is almost superfluous to remark on the very interesting field of experimental enquiry which this view lays open, in which however, little progress can be ex-

pected till more rigorous means have been devised of insulating the different homogeneous rays, so as to secure their absolute identity at all times, and under all circumstances, a subject to which I have already devoted some attention, and not altogether without success.

16. In the choice of media, then, for a double object-glass, we must be directed by the condition that their dispersive powers of the first order shall differ considerably. The equation

$$o = Lp + L'p',$$

the only one we can satisfy rigorously in this case, gives

$$\frac{L'}{L} = -\frac{p}{p'}$$

indicating that the lenses must be of opposite characters, and having their focal lengths in the direct ratio of their dispersions. If we call l the power of the compound lens, and take $\varpi = \frac{p}{p'}$, the ratio of the dispersions, we get

$$L = \frac{l}{1-\varpi},\ L = -\frac{l\varpi}{1-\varpi}$$

We have then only one farther guide to direct us in our choice of media, *viz.* that the dispersive powers of superior orders shall follow as nearly as possible the same proportion in both media, as those of the first.

17. In triple object-glasses we have

$$o = Lp + L'p' + L''p''$$

$$o = Lq + L'q' + L''q''$$

and $$l = L + L' + L''$$

whence we obtain

$$L = l \cdot \frac{p'q''-q'p''}{p(q'-q'')+p'(q''-q)+p''(q-q')}; \qquad (s)$$

$$L' = l \cdot \frac{p''q-q''p}{p(q'-q'')+p'(q''-q)+p''(q-q')}; \qquad (s')$$

$$L'' = l \cdot \frac{pq'-qp'}{p(q'-q'')+p'(q''-q)+p''(q-q')}; \qquad (s'')$$

or, $$\frac{\mathrm{L}}{\mathrm{L}'} = -\frac{\frac{p'}{p''} - \frac{q'}{q''}}{\frac{p}{p''} - \frac{q}{q''}}, \quad \frac{\mathrm{L}'}{\mathrm{L}''} = -\frac{\frac{p''}{p} - \frac{q''}{q}}{\frac{p'}{p} - \frac{q'}{q}}$$

In order then that this construction should be applicable to any useful purpose, the media must be such as to give moderate values to L, L′, L″, which will (generally speaking) be insured, provided none of the quantities $\frac{p}{p'}$, $\frac{p}{p''}$, $\frac{p'}{p''}$, approach very near in magnitude to the corresponding values $\frac{q}{q'}$, $\frac{q}{q''}$, $\frac{q'}{q''}$, or in other words, provided the media differ considerably in the scales of their dispersive powers.

Developement and application of the equations for correcting the spherical aberration.

18. The reciprocal distance of the focus of any combination of lenses or spherical surfaces from the posterior surface, is universally resolvable into a series of the form

$$\mathrm{M} + \mathrm{N}y^2 + \mathrm{O}y^4 + \mathrm{P}y^6 + \&c.$$

where y is the semi-aperture, or distance of the point of the first surface on which the ray falls, from the axis. In order then that this may be rigorously the same for rays incident on every point of the surface, this must be independent of y, and of course we must have

$$\mathrm{N} = o, \quad \mathrm{O} = o, \quad \mathrm{P} = o, \ \&c.$$

In ordinary telescopes however, y is sufficiently small to admit of our neglecting its fourth and higher powers with perfect impunity. Taking the focal length of the telescope for unity, if we allow an inch of aperture to every foot of focal length, we shall have $y = \frac{1}{24}$, $y^4 = \frac{1}{331776}$, &c. So that the remaining terms may be safely neglected in our present en-

quiry, and I shall accordingly confine myself to the equation $N = o$, or $\Delta f = o$.

If we develope this equation, it will assume the form

$$o = S + T . D + U . D^2$$

where S, T, U are functions of the curvatures and powers of the lenses. Now as the telescope may be directed to objects at all different distances, D is arbitrary and independent, and in consequence, the above equation must be satisfied, if possible, independent of D. This gives the three equations $S = o$, $T = o$, $U = o$, or, obtaining the values of these quantities from our equation *(o)*

$$\left.\begin{aligned} o = {} & \mu^2 L A \\ & + \mu'^2 L' \left\{ A' - B' L + C' L^2 \right\} \\ & + \mu''^2 L'' \left\{ A'' - B'' (L + L') + C'' (L + L')^2 \right\} \\ & + \&c. \end{aligned}\right\} ; \qquad (t)$$

$$\left.\begin{aligned} o = \mu^2 L B + \mu'^2 L' \left\{ B' - 2 C' L \right\} & \\ + \mu''^2 L'' \left\{ B'' - 2 C'' (L + L') \right\} & \\ + \&c. & \end{aligned}\right\} ; \qquad (u)$$

$$o = \mu^2 L C + \mu'^2 L' C' + \mu''^2 L'' C'' + \&c. \qquad (v)$$

19. Let us first consider the equation *(t)*. If we put for A, A′, B′, &c. their values in terms of r_1, r_2, r_3, &c. and moreover if we suppose

$$r_1 = r, r_3 = r', r_5 = r'', \&c.$$

and $$r_1 - r_2 = \varrho, r_3 - r_4 = \varrho', r_5 - r_6 = \varrho'', \&c.$$

$$\mu^2 (\alpha + \alpha' + \alpha'') = a, \mu^2 (\alpha' + 2 \alpha'') = b, \mu^2 \alpha'' = c$$

$$\mu^2 (\beta + \beta') = e, \mu^2 \beta' = f, \mu^2 \gamma' = g$$

and similarly for the other lenses, accenting the letters *a*, *b*, *c*, &c. the equation will become

$$o = L\left\{ar^2 - b\varrho r\right\} + L'\left\{a'r'^2 - (b'\varrho' + Le')\,r'\right\}$$
$$+ L''\left\{a''r''^2 - (b''\varrho'' + (L + L')e'')\,r''\right\}$$
$$+ \&c.$$
$$+ Lc\varrho^2 + L'c'\varrho'^2 + L''c''\varrho''^2 + \&c.$$
$$+ L'Lf'\varrho' + L''(L + L')f''\varrho'' + \&c.$$
$$+ L'L^2g' + L''(L + L')^2g'' + \&c.$$

and finally, substituting in this equation for ϱ, ϱ', &c. their values $\frac{L}{\mu - 1}$, $\frac{L'}{\mu' - 1}$, &c., and for a, b, c, e, f, g, &c. their values deduced from the equations of Art. 8, we obtain; (w)

$$o = L\left\{(2m + 1)r^2 - \frac{2\mu + 1}{\mu - 1}Lr\right\}$$
$$+ L'\left\{(2m' + 1)r'^2 - \left((4m' + 4)L + \frac{2\mu' + 1}{\mu' - 1}L'\right)r'\right\}$$
$$+ L''\left\{(2m'' + 1)r''^2 - \left((4m'' + 4)(L + L') + \frac{2\mu'' + 1}{\mu'' - 1}L''\right)r''\right\}$$
$$+ \&c.$$
$$+ \frac{\mu^2L^3}{(\mu - 1)^2} + \frac{\mu'^2L'^3}{(\mu' - 1)^2} + \frac{\mu''^2L''^3}{(\mu'' - 1)^2} + \&c.$$
$$+ \frac{3\mu' + 1}{\mu' - 1}LL'^2 + \frac{3\mu'' + 1}{\mu'' - 1}(L + L')L''^2 + \&c.$$
$$+ (2m' + 3)L^2L' + (2m'' + 3)(L + L')^2L'' + \&c.$$

In this equation it will be observed, the quantities r, r', &c. relative to the several lenses are not combined with each other by multiplication, nor do they rise above the second degree. If then we assume, or determine from other conditions the powers L, L, &c. the equation takes a form of great simplicity. Now, as we have already seen, the destruction of the chromatic aberration depends on relations between the focal lengths only, without any regard to the curvatures of the surfaces, and therefore furnishes equations tending to this very point. It is a singular circumstance, and it cannot but be regarded as a very fortunate one, that the introduction of

another condition quite independent of the correction of the spherical aberration, and which at first sight seems likely greatly to increase the difficulty of the investigation, should on the contrary tend so remarkably to simplify it.

In general, when the focal lengths are assumed, there will be as many unknown quantities r, r', &c. as there are lenses, and the aberration for parallel rays may therefore be destroyed in a great variety of ways, some more, some less advantageous. If, for example, we limit the figure of one of the lenses in any way (as if we assume it plano-convex or concave,) or assign equal curvatures to both its surfaces, &c. such limitation is equivalent to assigning given values to both its radii, and the terms depending on that lens in equation (w) pass into the given part of the equation.

20. Let us next consider the equations (u) and (v). The latter does not involve the radii of the lenses, but only their powers, being in fact when developed.

$$o = (2m+3)\,L + (2m'+3)\,L' + (2m''+3)\,L'' + \&c.; \quad (x)$$

In a double object-glass, this equation will be incompatible with the equation $o = p\,L + p'\,L'$ expressing the condition of achromaticity, and must of course be sacrificed, the latter being of paramount importance.* It is in fact a very secondary consideration to satisfy this condition in telescopes. In the microscope, however, where D is necessarily a quantity

* Unless such a peculiar adjustment of the media should take place as to render the two conditions identical, which would give

$$\frac{p}{2m+3} = \frac{p'}{2m'+3} \text{ or } \frac{p\mu}{3\mu+2} = \frac{p\,\mu'}{3\mu'+2}.$$

It is a mere matter of curiosity to look for media satisfying this equation. Fluor spar combined with rock crystal comes very near it, but among bodies adapted for object-glasses there are probably none to be found except such as would result from mixtures of different liquids.

of the same order with the powers and curvatures of the glasses, it may be a matter of some moment.

21. The equation (u) being developed becomes

$$\left.\begin{aligned} o = &(4m+4)\,\mathrm{L}r + (4m'+4)\,\mathrm{L}'r' + (4m''+4)\,\mathrm{L}''r'' + \&c. \\ &- \left\{\tfrac{3\mu+1}{\mu-1}\mathrm{L}^2 + \tfrac{3\mu'+1}{\mu'-1}\mathrm{L}'^2 + \tfrac{3\mu''+1}{\mu''-1}\mathrm{L}''^2 + \&c.\right\} \\ &- \left\{(4m'+6)\,\mathrm{LL}' + (4m''+6)\,(\mathrm{L}+\mathrm{L}')\,\mathrm{L}'' + \&c.\right\} \end{aligned}\right\}; \quad (y)$$

which being of the first degree, adds nothing to the algebraic difficulty of the problem.

22. Let us apply these results to the case of a double object-glass, and putting, as in Art. 16, ϖ for the ratio of the dispersive powers, and writing for L and L′ their values $\frac{l}{1-\varpi}$ and $-\frac{\varpi l}{1-\varpi}$, the equation (w) becomes,

$$\begin{aligned} o = &(2m+1)\,r^2 - \tfrac{2\mu+1}{\mu-1} \cdot \tfrac{1}{1-\varpi} \times l r \qquad (z) \\ &+ \left\{\left(\tfrac{\mu}{\mu-1}\right)^2 - (2m'+3)\varpi + \tfrac{2\mu'+1}{\mu'-1}\varpi^2 - \left(\tfrac{\mu'}{\mu'-1}\right)^2\varpi^3\right\}\tfrac{l^2}{(1-\varpi)^2} \\ &+ \left\{(4m'+4) - \tfrac{2\mu'+1}{\mu'-1}\varpi\right\} \cdot \tfrac{\varpi}{1-\varpi}\, l r' - \varpi\,(2m'+1)\,r'^2 \end{aligned}$$

while (y) reduces itself to

$$\left.\begin{aligned} o = &(4m+4)\,r - (4m'+4)\,\varpi \cdot r' \\ &- \left\{\tfrac{3\mu+1}{\mu-1} - (4m+6)\varpi + \tfrac{3\mu'+1}{\mu'-1}\varpi^2\right\}\tfrac{l}{1-\varpi} \end{aligned}\right\}; \qquad (A)$$

To reduce these equations into numbers, we may observe that the value of ϖ is that which varies within the most considerable limits. If we combine the least dispersive flint with the most dispersive crown or plate glass which have yet been observed by DOLLOND, BOSCOVICH, ROBISON, BREWSTER, &c. and *vice versâ*, we shall find 0·51 and 0·782 for the minimum and maximum of this quantity; but it is rare to meet with the extremes. Mr. TULLEY was so good as to communicate to me the highest and lowest dispersions of the two sorts of glass, which had occurred to him in the

course of his practice, and calculating on these, I find 0·56915 and 0·65617 for the corresponding values of ϖ, so that we may fairly take 0·60 for its average value. With regard to μ and μ', their limits are much narrower. In crown and plate glass, we have 1·504 and 1·544 for the extreme indices, while in flint 1·5735 and 1·625 are the lowest and highest I have met with any account of. TULLEY's extremes are 1·5735 and 1·599, and it is said to be very rare at present to meet with flint higher than 1·600. In one specimen only have I observed a greater refraction. We may therefore fix on 1·524 and 1·585 for the mean or average values of μ and μ'. In order however to embrace a greater range of the function, should we be desirous of interpolating, as well as to provide for the possible discovery of a mode of making flint glass of high dispersive power free from veins, (a thing which it seems very reasonable to *hope*, and which the recent liberality of Government in affording facilities to experiments on a large scale for this express purpose, gives us some ground to *expect*) I have computed the coefficients of our equations (z) and (A) for values of ϖ from 0·50 to 0·75 inclusive, and the results are presented in the following tables.

TABLE I. Coefficients of equation (z), for correcting the spherical aberration for rays parallel to the axis. $\begin{cases} \mu = 1{\cdot}524 \\ \mu' = 1{\cdot}585 \end{cases}$

$\varpi = 0{\cdot}50$	$2{\cdot}3123 \times r^2$	$-15{\cdot}4505 \times lr$	$+\ 31{\cdot}4786 \times l^2$	$+2{\cdot}9596 \times lr'$	$-1{\cdot}1309 \times r'^2 = 0$
0·55	2·3123	−17·1671	+ 38·8609	+3·1817	−1·2440
0·60	2·3123	−19·3130	+ 49·1100	+3·3702	−1·3571
0·65	2·3123	−22.0720	+ 63·9098	+3·5107	−1·4702
0·70	2·3123	−25·7503	+ 86·4217	+3·5703	−1·5833
0·75	2·3123	−30·9007	+123·1862	+3·5326	−1·6963

TABLE 2. Coefficients of equation (A) for correcting the aberration of diverging rays, μ and μ' as in Table 1.

$\varpi = 0.50$	$6.6247 \times r$	$-17.6627 \times l$	$-3.2618 \times r' = 0$
0.55	6.6247	−19.8254	−3.5880
0.60	6.6247	−22.6523	−3.9142
0.65	6.6247	−26.4274	−4.2404
0.70	6.6247	−31.6245	−4.5666
0.75	6.6247	−39.0979	−4.8928

23. If these equations be combined, we shall obtain the dimensions of an object-glass free from aberration, both for celestial and terrestrial objects, provided we restrict our views to objects situated in the prolongation of the axis of the telescope. The arithmetical operations necessary for determining the values of r and r' being executed, we shall find that the resulting quadratics admit real roots, and that in consequence there are two sets of curvatures assignable to the surfaces, which satisfy the algebraic conditions. The values of the first set are however objectionable, as they will be found to correspond to meniscus and concavo-convex forms of the crown and flint lenses respectively, of great curvature, and placed together as in Pl. XIX. fig. 7. Those of the other correspond to moderate curvatures and very convenient forms, as represented in Pl. XIX. fig. 8. The values of r, r', in this series, and those of r_2 and r_4 the curvatures of the posterior surfaces, deduced from them as well as those of L, L', the powers of the lenses are set down in the subjoined table, in which (for simplicity) we have taken $l = 1$.

TABLE 3. Values of r, r' &c. deduced from equations (z), (A). $\begin{cases} \mu = 1\cdot524 \\ \mu' = 1\cdot585 \end{cases}$

$\varpi =$	r or $r_1 =$	$r_2 =$	r' or $r_3 =$	$r_4 =$	L=	L′=
0·50	+1·4818	−2·3350	−2·4053	−0·6959	+2·0000	−1·0000
0·55	1·4885	2·7524	2·7772	0·6880	2·2222	1·2222
0·60	1·4910	3·2800	3·2637	0·6996	2·5000	1·5000
0·65	1·4855	3·9670	3·9115	0·7369	2·8571	1·8571
0·70	1·4646	4·8967	4·8005	0·8120	3·3333	2·3333
0·75	1·4121	6·2215	6·0790	0·9508	4·0000	3·0000

24. These values once obtained, it is easy to calculate the radii and focal lengths of the respective lenses, which I have accordingly set down, for the convenience of those who may be inclined to make trial of this construction, as follows.

TABLE 4. Dimensions of an aplanatic double object glass, indices of refraction 1·524 (crown) and 1·585 (flint). Compound focal length 10·0000.

Ratio of the dispersive powers.	Radius of the 1st. surface. +	Radius of the 2d. surface. —	Radius of the 3d. surface. —	Radius of the 4th. surface. —	Focal length of the crown lens. +	Focal length of the flint lens. —
0·50:1	6·7485	4·2827	4·1575	14·3697	5·0	10·0000
0·55	6·7184	3·6332	3·6006	14·5353	4·5	8·1818
0·60	6·7069	3·0488	3·0640	14·2937	4·0	6·6667
0·65	6·7316	2·5208	2·5566	13·5709	3·5	5·3846
0·70	6·8279	2·0422	2·0831	12·3154	3·0	4·2858
0·75	7·0816	1·6073	1·6450	10·5186	2·5	3·3333

To reduce these values to those required for any other proposed focal length of the compound lens, a simple proportion is all that is necessary.

25. This table and the preceding afford room for one or two remarks of some moment. And, first, with regard to the

putting together of the lenses, it will be observed that for the lower values of ϖ, or the more dispersive varieties of flint-glass, the curvature of the third surface is a very little greater than that of the second, so that the glasses when laid together in their proper position will have a minute interval between them. At a certain value of ϖ between 0·55 and 0·60 (ϖ=0·58 nearly) this interval vanishes, and the glasses are in contact over their whole surface. For higher dispersive ratios, if laid close together, they would touch in the vertex. This is regarded as an objection in practice, and justly, (especially when the curvatures of the surfaces in contact differ considerably) as their pressure on each other at the centre must tend to distort their figures, and disturb the uniformity of their density, not to speak of the production of the colours of thin plates, whose effect on vision is more problematic. But in fact, the difference of the curvatures in this construction is so very trifling, as to fall within the limits of practical errors, and therefore, if the separation of the two glasses by a ring of metal (which in a 10 feet object-glass, of 5 inches aperture, even in the very unfavourable case of ϖ=0·70 need not exceed 1-400th of an inch in thickness) be deemed unadvisable, it may be neglected, and the glasses ground to the same radius, provided only the necessary alterations are made in the other surfaces to preserve the proper proportion of their focal lengths.

26. With regard to the interpolation of the tables above given for intermediate values of ϖ; if we cast our eyes down the second and 5th columns, we cannot but be struck by the very small alteration in the values of r_1 and r_4, the curvatures of the first and last surfaces throughout the whole *useful*

extent of the table, i. e. as far as $\varpi = 0{\cdot}70$, (beyond which it is very unlikely it should ever extend in practice). In fact, these values have, the one a maximum and the other a minimum between $\varpi = 0{\cdot}55$ and $\varpi = 0{\cdot}60$. The principal variation takes place on the values of r_2 and r_3, which change rapidly as ϖ increases, the whole stress of the adjustment by which the aberration is corrected being laid on these surfaces. We may take advantage of this fortunate and very remarkable circumstance, and assigning to r_1 and r_4 constant values, such as to give the least average error, employ them to complete the interior curvatures: thus we may announce it as a practical theorem, which in all probability will be found sufficiently exact for use, that *a double object-glass will be free from aberration, provided the radius of the exterior surface of the crown lens be* 6·720, *and of the flint* 14·20, *the focal length of the combination being* 10·000, *and the radii of the interior surfaces being computed from these data, by the formulæ given in all elementary works on optics, so as to make the focal lengths of the two glasses in the direct ratio of their dispersive powers.*

27. It remains to examine the effect of a change of the values of μ and μ' on the curvatures. Now the variations to which these quantities are subject being very trifling, we may neglect their squares and products, and we shall have

$$dr = \frac{dr}{d\mu} d\mu + \frac{dr}{d\mu'} d\mu'$$

where $\frac{dr}{d\mu}$ and $\frac{dr}{d\mu'}$, are constant co-efficients, which are most readily computed by repeating the preceding calculations for values of μ and μ' a little differing from those before assumed. And first, with respect to $\frac{dr}{d\mu}$: if we take $\mu = 1{\cdot}504$ and

$\mu'=1\cdot585$, the resulting curvatures and radii will be as in the two following tables, to which we have also subjoined the values of the coefficients of (z) to save the trouble of recomputation, should any other equation beside (A) be thought preferable to use with it.

TABLE 5. Values of r, r', &c. $\left\{\begin{array}{l}\mu=1\cdot504\\ \mu'=1\cdot585\end{array}\right.$

$\varpi=$	r or $r_1=$	$r_2=$	r' or $r_3=$	$r_4=$
0·50	+1·5041	−2·4642	−2·5168	−0·8074
0·55	1·5220	2·8871	2·8880	0·7988
0·60	1·5217	3·4385	3·3916	0·8275
0·65	1·5108	4·1582	4·0636	0·8890
0·70	1·4791	5·1346	4·9893	1·0007
0·75	1·4052	6·5313	6·3259	1·1977

and the radii being calculated from these in the same manner as before, will come out as follows:

TABLE 6. Radii of an aplanatic object glass. Focal length = 10·0000. Refractive indices 1·504 and 1·585.

Ratio of dispersive powers.	Radius of the 1st. surface. +	Radius of the 2d. surface. −	Radius of the 3d. surface. −	Radius of the 4th. surface. −
0·50	6·6485	4·0581	3·9733	12·3854
0·55	6·5703	3·4637	3·4626	12·5193
0·60	6·57:6	2·9082	2·9484	12·0839
0·65	6·6190	2·4049	2·4608	11·2481
0·70	6·7609	1·9476	2·0043	9·9927
0·75	7·1164	1·5311	1·5808	8·3491

TABLE 7. Coefficients of the equation (z) for correcting the spherical aberration of rays parallel to the axis. $\begin{cases}\mu = 1\cdot504 \\ \mu' = 1\cdot585\end{cases}$

$\varpi = 0\cdot50$	$2\cdot3298 \times r^2$	$-15\cdot9048 \times lr +$	$33\cdot2638 \times l^2$	$+2\cdot9596 \times lr'$	$-1\cdot1309 \times r'^2 = 0$
0·55	2·3298	—17·6720	+ 41·0647	+3·1817	—1·2440
0·60	2·3298	—19·8810	+ 51·8989	+3·3702	—1·3571
0·65	2·3298	—22·7211	+ 67·5529	+3·5107	—1·4702
0·70	2·3298	—26·5077	+ 91·3803	+3·5793	—1·5833
0.75	2·3298	—31·8095	+130·3267	+3·5326	—1·6963

The differences between the numbers in tables 5 and 6, and the corresponding numbers in tables 3 and 4, divided by —0·020 (the value of $d\mu$) give the values of $\frac{dr}{d\mu}$, &c. and $\frac{dR_1}{d\mu}$ &c. calling R_1 R_2, &c. the radii of the respective surfaces. Thus we find

TABLE 8. Values of $\frac{dr_1}{d\mu}, \frac{dr_4}{d\mu}, \frac{dR_1}{d\mu}, \frac{dR_4}{d\mu}$

$\varpi =$	$\frac{dr_1}{d\mu} =$	$\frac{dr_4}{d\mu} =$	$\frac{dR_1}{d\mu} =$	$\frac{dR_4}{d\mu} =$
0·50	—1·115	+ 5·575	+5·000	+ 99·215
0·55	—1·675	+ 5·540	+7·405	+100·800
0·60	—1·535	+ 6·397	+6·675	+110·490
0·65	—1·265	+ 7·605	+5·630	+116·140
0.70	—0·725	+ 9·435	+3·350	+116·135
0·75	+0·345	+12·345	—1·740	+108·475

28. Instituting similar computations for the variation of μ' from 1·585 to 1·600, we shall obtain the following results.

TABLE 9. Values of r_1, r_2, r_3, r_4, $\begin{cases}\mu = 1\cdot524\\ \mu' = 1\cdot600\end{cases}$

$\varpi =$	$r_1 =$	$r_2 =$	$r_3 =$	$r_4 =$
0·50	+1·4830	−2·3338	−2·3926	−0·7259
0·55	1·4888	2·7521	2·7627	0·7257
0·60	1·4898	3·2812	3·2474	0·7438
0·65	1·4814	3·9711	3·8938	0·7986
0·70	1·4546	4·9066	4·7534	0·8945
0·75	1·3953	6·2383	6·0597	1·0597

TABLE 10. Values of the radii $\begin{cases}\mu = 1\cdot524\\ \mu' = 1\cdot600\end{cases}$

$\varpi =$	$R_1 =$	$R_2 =$	$R_3 =$	$R_4 =$
0·50	+6·7431	−4·2849	−4·1795	−13·7754
0·55	6·7168	3·6336	3·6196	13·7803
0·60	6·7125	3·0477	3·0794	13·4448
0·65	6·7503	2·5182	2·5682	12·5224
0·70	6·8747	2·0381	2·0906	11·1799
0·75	7·1668	1·6030	1·6503	9·4375

TABLE 11. Coefficients of the equation (z) for correcting the spherical aberration of rays parallel to the axis $\mu = 1\cdot524$, $\mu' = 1\cdot600$.

$\varpi = 0\cdot50$	$2\cdot3123 \times r^2$	$-15\cdot4505 \times lr$	$+\ 31\cdot4462 \times l^2$	$+3\cdot0000 \times lr'$	$-1\cdot1250 \times r'^2 = 0$
0·55	2·3123	−17·1671	+ 38·8263	+3·2389	−1·2375
0·60	2·3123	−19·3130	+ 49·0798	+3·4500	−1·3500
0·65	2·3123	−22·0720	+ 63·8984	+3·6214	−1·4625
0·70	2·3123	−25·7503	+ 86·4584	+3·7333	−1·5750
0·75	2·3123	−30·9007	+123·3404	+3·7500	−1·6875

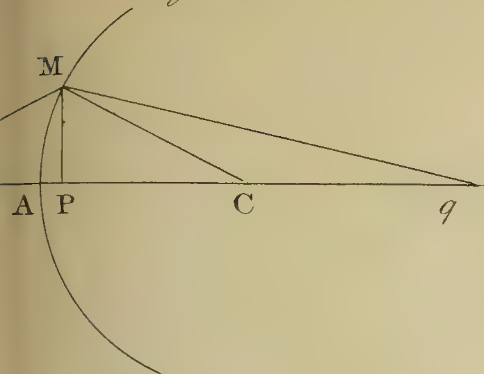


Fig. 1.

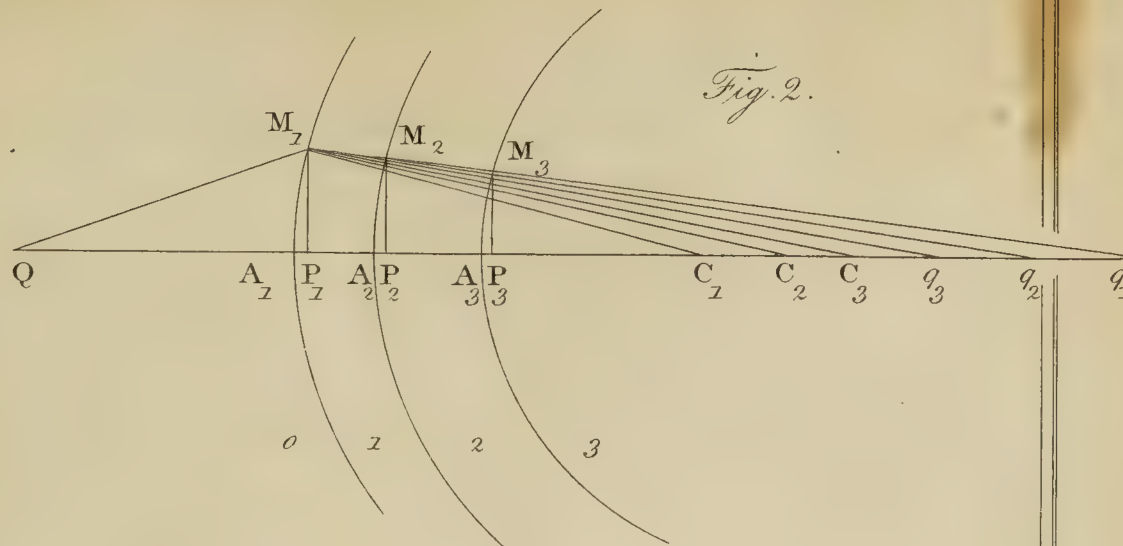


Fig. 2.

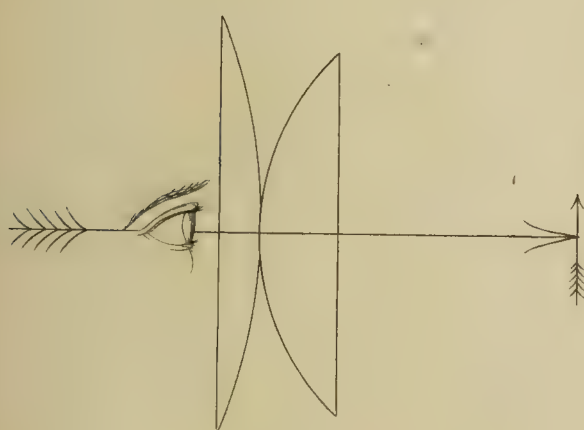
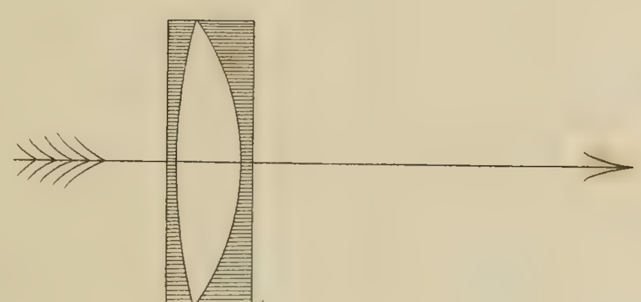


Fig. 3.



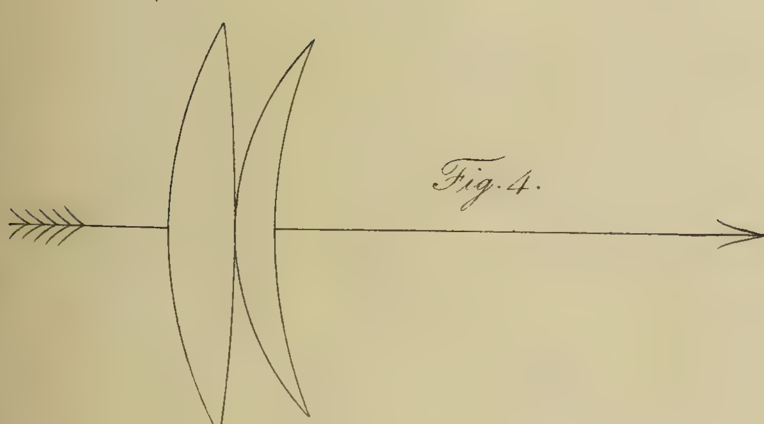


Fig. 4.

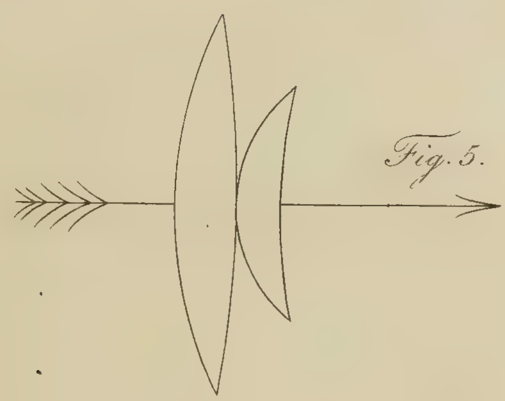


Fig. 5.

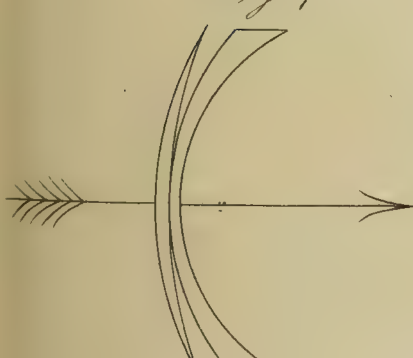


Fig. 7.

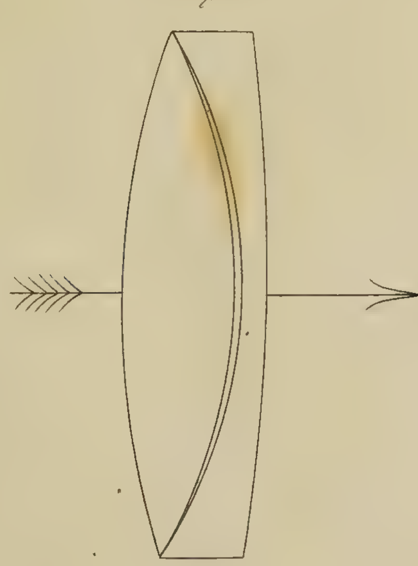


Fig. 8.

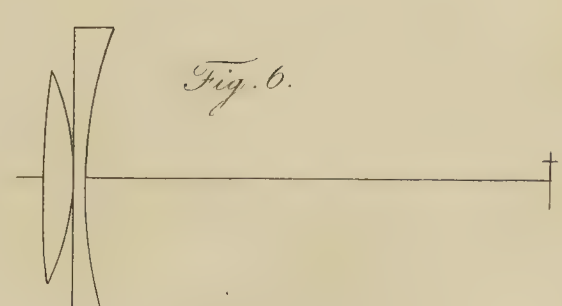


Fig. 6.

TABLE 12. Values of $\frac{dr_1}{d\mu'}, \frac{dr_4}{d\mu'}, \frac{dR_1}{d\mu'}, \frac{dR_4}{d\mu'}$.

$\varpi=$	$\frac{dr_1}{d\mu'}=$	$\frac{dr_4}{d\mu'}=$	$\frac{dR_1}{d\mu'}=$	$\frac{dR_4}{d\mu'}=$
0·50	+0·080	−2·000	−0·360	+39·620
0·55	+0·020	−2·520	−0·107	+50·333
0·60	−0·080	−2·947	+0·373	+53· 16
0·65	−0·273	−4·113	+1·180	+69·900
0·70	−0·667	−5·500	+3·133	+75·700
0·75	−1·120	−7·253	+5·680	+72·083

It will be seen by this statement, that the variations of the curvatures arising from a variation in the refractive power of the flint lens, are much smaller than those produced by an alteration in that of the crown, which is another fortunate circumstance, the crown and plate glass usually met with being much more uniform in this respect than the flint.

JOHN F. W. HERSCHEL.

Slough, Feb. 19, 1821.

XVIII. *An account of the skeletons of the dugong, two-horned rhinoceros, and tapir of Sumatra, sent to England by Sir* Thomas Stamford Raffles, *Governor of Bencoolen. By Sir* Everard Home, *Bart. V. P. R. S.*

Read March 22, 1821.

Judging from the exertions Sir Thomas Raffles has already made in promoting the pursuits of Natural History and Comparative Anatomy, during the short time he has been in Sumatra, we may at no distant period expect to be furnished with materials sufficient to give a most satisfactory account of all the natural productions of the Island.

In the interval between his account of that extraordinary animal the dugong, being read before the Society, and its being inserted in the Transactions, he has afforded fresh proofs of his exertions, and has sent me the skull, the viscera, and the bones of that animal; so that, in addition to the account of its internal organs illustrated by drawings, I am now enabled to give an exact representation of the skeleton by Mr. Clift, upon the same scale as that of the external appearance of the animal, which has a place in a former paper, which is two inches to a foot.

Sir T. S. Raffles' description of this animal was so clear and distinct, that a Memoir since read to the Society, written by two French naturalists employed under his direction, was so nearly the same, as to make it superfluous to have it published.

The bones of the skeleton, when mounted, give us a form very different from what is met with in the whale tribe. It will be seen from the annexed drawing, that it may be compared to a boat without a keel, with the bottom uppermost; so that in the sea, the middle part of the back is the highest point in the water; and as the lungs are extended to great length on the two sides, close to the spine, they furnish the means of the animal becoming buoyant, and when no muscular exertion is made, the body will naturally float in an horizontal posture.

When we consider that this animal is the only one yet known that grazes at the bottom of the sea, (if the expression may be allowed), and is not supported on four legs, we must admit that it will require a particular mode of balancing its body over the weeds upon which it feeds.

The hippopotamus, an animal that uses the same kind of food, from the strength of its limbs supports itself under water; and the dugong, as a compensation for not being able to support its body on the ground, has this means of steadily suspending itself in the sea peculiar to itself, the centre of the back forming the point of suspension, similar to the fulcrum of a pair of scales. This peculiarity of position explains the form of the jaws, which are bent down at an angle with the skull, unlike the jaws of other animals. This new mode of floating, when compared with that of other sea animals, makes a beautiful variety. The balæna mysticetus, that goes to the bottom of unfathomable depths to catch in its whale-bone net the shrimps that live in that situation, is surrounded by blubber not unlike a cork jacket.

The enormous spermaceti whale, whose prey is not so

far removed from the surface, has the mass of spermaceti in a bony concavity upon the skull.

The shark tribe have the liver loaded with oil, placed in nearly the same situation as the lungs of the dugong.

As there are no vegetables (I believe) growing at the bottom of the sea in very deep water, the nice adjustment of the body of the dugong is confined to the shallows in the creeks near the land.

The external appearance of the rhinoceros with two horns, from Sumatra, is described in the Philosophical Transactions for 1793, by my much lamented friend Mr. Bell; and drawings are given both of the entire animal, and the skull with the teeth; but till now I have not had an opportunity of examining the rest of the skeleton. Upon comparing the bones with those of the single-horned species, there is no difference deserving of particular remark, except that in the two-horned the projection towards the front of the skull formed by the union of the nasal bones, is more nearly in a straight line, and more extended; this peculiarity may be required to give sufficient surface for two horns. A drawing of the skeleton, which Mr. Hills has been so obliging as to make for me, is annexed.

In the internal viscera, there is not that close resemblance which is met with in the skeleton. Mr. Thomas, in Vol. 91 of the Philosophical Transactions, describes the stomach of the species he dissected, to be, in its external appearance, as well as the intestinal canal, similar to that of the horse, only the cœcum was much larger; but the lining of the stomach was every

where villous. The small intestines, which were short, had oblong processes from the internal membrane. There was no gall bladder, and the kidneys conglomerate, large, and flattened, but less so than in the bear.

In the rhinoceros from Sumatra, which is four feet seven inches and a half high at the shoulder, and eight feet from the nose to the rump, the œsophagus enters the stomach ten inches from its cardiac extremity; the internal membrane is smooth round its great curvature; from the œsophagus to the pylorus five feet nine inches. The extent of the cuticular lining is shown in the drawing. In shape altogether the stomach is nearer that of the elephant, but in its cuticular portion is similar to the horse; and a bott, in all respects the same as those met with in this country in horses, was found in it.

The small intestines measured fifty-four feet six inches; the valvulæ conniventes are continued nearly through the whole extent, and in general circular, although not all so.

The cæcum is conical, two feet six inches long, one foot six inches wide, irregularly honey-combed, and has some of the conical processes delineated by Mr. Thomas. The colon and rectum are twenty-six feet long. From the termination of the longitudinal bands to the anus is eight feet six inches of that length. The spleen is long, thin, and flat, two feet long, and at the broadest part eleven inches wide.

The kidneys are conglobate, and rather longer than common.

The heart is short and rounded; in other respects as usual in the class mammalia.

While this paper was printing, I was invited by the Trea-

surer of the Missionary Society, in the Old Jewry, to see the horns of a double-horned rhinoceros, brought from the interior of Africa by Mr. CAMPBELL, whose travels will soon be laid before the public. As far as respects the appearance of the horns, it is intirely a new species. The lowest horn does not, as in the other species of this animal, both single and double-horned, stand upon the upper surface of the nasal bones, pointing upwards, but it is set on upon a projection, as it were, on the end of these bones, standing with its base nearly horizontal, pointing forwards and a little upwards; in this respect a true unicorn. It is a yard long, very small at the point, and two feet in circumference at the base. The small horn is close to it, and stands up perpendicularly behind the base of the long one, as if it were to give it support, and is only twelve inches high, while the circumference of its base is twenty-four inches.

There can be no doubt of this being the animal that has given rise to various reports of a true unicorn having at last been discovered in Africa.

The tapir of Sumatra, as well as that of America, have a greater general resemblance to the rhinoceros than to any other animal.

When the bones of these two species of tapir are compared, they are found very closely to resemble one another. The skull of that of Sumatra has a broader frontal bone, and no middle ridge; the two nasal bones, which in both species have the shape of a heart on cards, stand higher, and are

broader, making the openings of the nostrils larger. In the American, the parietal bones are much compressed, and the os frontis has a considerable ridge.

When the bones of these tapirs are compared with those of the rhinoceros, they are in general alike, except the scapulæ and pelvis, which have a less comparative extent of surface.

The tapir has seven molares above and six below; the rhinoceros only six above and below. In the molares of the tapir the broad outside plate of those of the rhinoceros is wanting, but the deep indentations on the inside are nearly alike.

The large bony process projecting from the outside of the thigh bone, so conspicuous in the rhinoceros, is equally so in both species of tapir, and is much smaller in the horse.

The Sumatra tapir has a stomach in shape very much like that of the rhinoceros; it is one foot eight inches long. The œsophagus is smooth and cuticular, the cuticle terminating round the entrance into the stomach in an oval form; the stomach in its long axis resembles that of the hog; its greatest breadth nine inches; the internal membrane smooth and villous.

The small intestines are sixty-nine feet long. The valvulæ conniventes do not extend so far down as in the rhinoceros; the surface is villous towards the cœcum. The length and greatest breadth of the cœcum is one foot; internally it is honeycombed, and has conical projections like those found by Mr. Thomas in the small intestines. The cœcum is shorter than in the rhinoceros, and conical. The colon is about three feet from the cœcum, dilates considerably, and for about

two feet is eight inches in diameter, resembling a stomach; it then becomes as small as before; the colon and rectum are nineteen feet six inches long. The rectum is one foot three inches of that length. The colon appeared to have only one longitudinal band, which was most distinct towards its termination.

The spleen is long and narrow, two feet three inches long, from two to three inches wide.

The kidneys are conglobate.

The lungs are composed of one principal lobe on each side of considerable length; and from this there are two projections, or smaller lobes, one passing upwards, the other rather downwards, on the inside of the large ones.

EXPLANATION OF THE PLATES.

Plate XX.

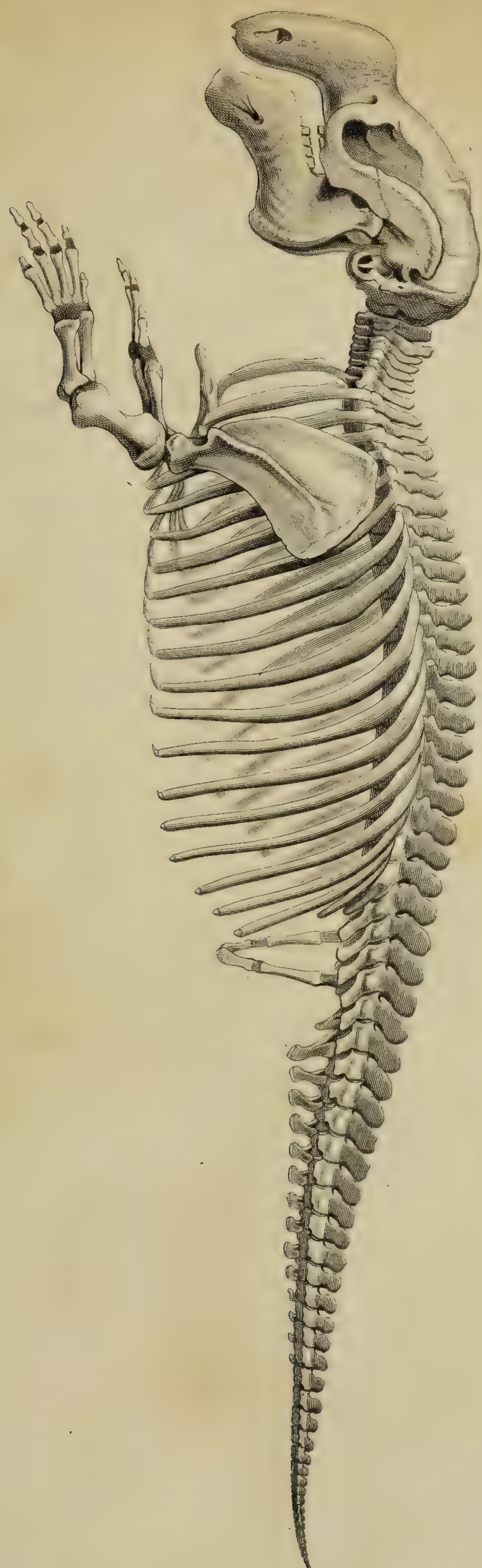
The skeleton of a small female dugong. Upon a scale of two inches to a foot.

Plate XXI.

The stomach of the rhinoceros from Sumatra inverted, to show the extent of the cuticular lining, in all respects exactly resembling that of the horse. Natural size.

Plate XXII.

The skeleton of the rhinoceros from Sumatra, which closely resembles the species from India, except that the point in which the nasal bones of the skull terminate, is rather more prominent. On the scale of one inch to a foot.



Wm. Clift del.

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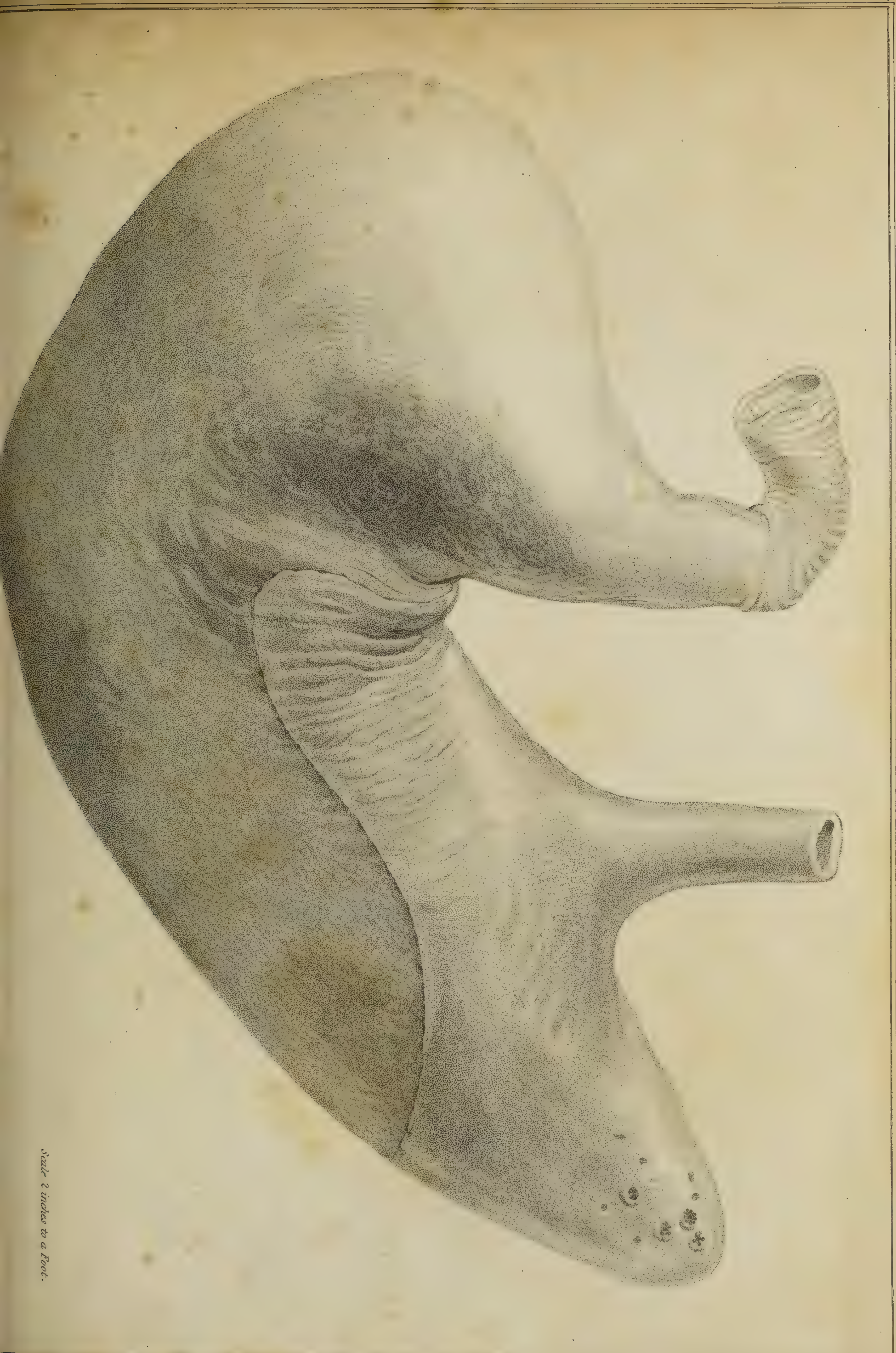

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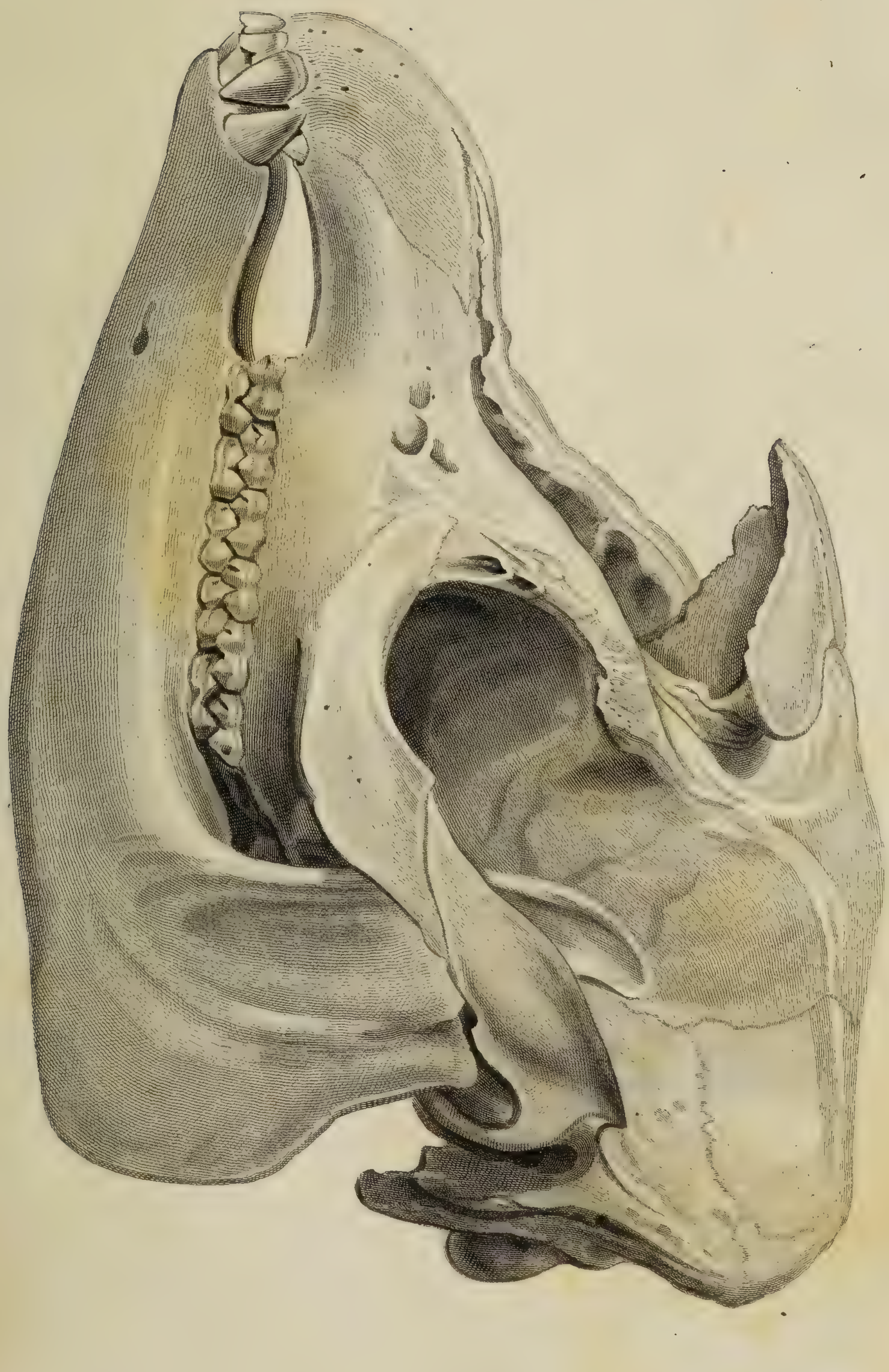

Scale Four inches to a Foot.

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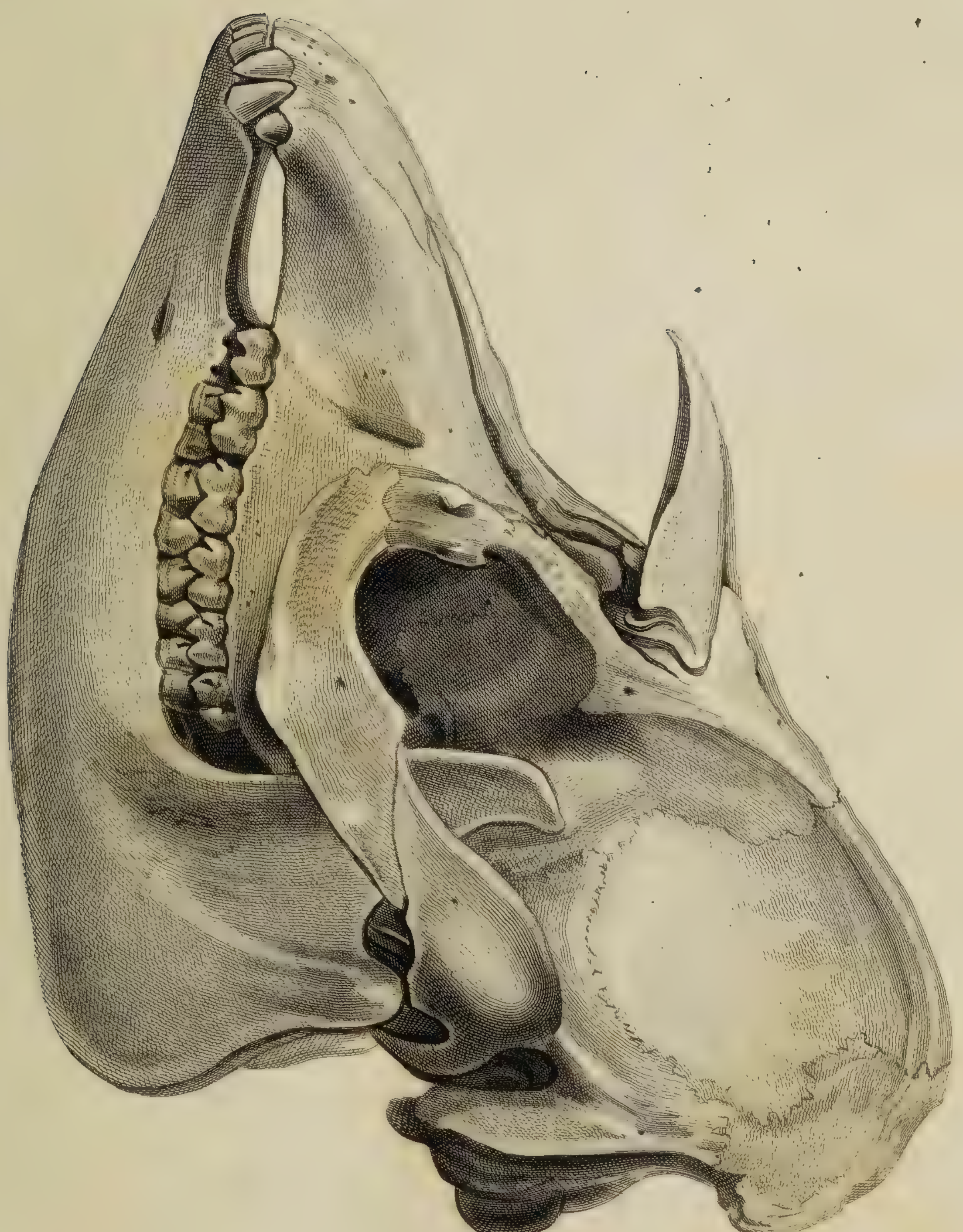

Scale Four inches to a Foot.

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Plate XXIII.

The skull of the tapir from Sumatra, which is a nearer approach to that of the rhinoceros than the hog, particularly in the termination of the nasal bones, although in this animal they are considerably broader. On the scale of four inches to a foot.

Plate XXIV.

The skull of the tapir from America, to show in what it differs from that of Sumatra. On the same scale as plate XXIII.

XIX. *On the mean density of the Earth.* *By Dr.* Charles Hutton, *F. R. S.*

Read April 5, 1821.

Although the determination of the mean density of the whole terraqueous globe of our planet, is admitted to be a problem of the utmost importance to several branches of philosophy, particularly to physical astronomy, and the figure and constitution of the earth; it would seem, from the discordancy of the declared opinions of some eminent philosophers, that the problem is still in an uncertain state. Since the first notice of this subject by Newton, in his admirable Principia, it has often been incidentally alluded to, without receiving a precise determination; with the exception of two instances only, in which it has been stated to be, certainly or approximately, determined by experiment; namely, in the case of the Schehallien experiment, by Dr. Maskelyne and myself, and by the Honorable Henry Cavendish, by a method invented by Mr. Michell.

The former of these experiments was made by Dr. Maskelyne, in the years 1774, 1775, and 1776, by means of that large mountain in Scotland, in measuring its dimensions, and in comparing its attraction on a plummet, with that of the whole earth on the same; the calculations on it having been made by myself, and first published in the Philosophical Transactions of the year 1778; and since more correctly in the second volume of my Mathematical Tracts.

The other experiment, by Mr. Cavendish, was by observing the attraction on small pendulous balls, of two inches diameter, by larger ones of ten inches diameter, as compared with the attraction of the earth on the same.

By some strange mistake, or perversion, for many years, it was customary among certain persons, to withhold the mention of my name, with regard to the great share that I had in the experiment on Schehallien. But from certain complaints which I have made, some little justice has lately been awarded to me on that head; though still, it would seem, with reluctance, as the opinion is promptly assumed that the latter small experiment is susceptible of the greater accuracy, and the numbers in its result gratuitously adopted as nearer the truth than that of the former. As this is an opinion which I have never been able to bring my mind to acknowledge, and as it is a matter of great importance in the present state of physics, I have been desirous to draw the attention of philosophers to a closer consideration of the subject, with a view to a more deliberate and impartial decision of this point.

From the closest and most scrupulous attention I can employ on this question, the preference, in point of accuracy, appears to be decidedly in favour of the large or mountain experiment, over that of the small balls. It is indeed true, that though the large mass of the mountain must yield an immensely greater force than a small ball, yet it may be said that this advantage must be balanced, either wholly or in great part, on the score of distance, as the plummet is acted on at a great distance from the centre of the mountain, while the balls are approached very near together; so that

the visible effects may thus be nearly equal, by the reciprocal balancing between magnitude and distance. Hence the visible effect of the mountain, is that of the small angle of eleven or twelve seconds, by which the plummet is drawn aside from the perpendicular; thereby showing that the attraction of the earth, on the plummet, is to that of the mountain on it, as radius is to the tangent of those seconds; while, in the other experiment, the small pendulous balls are drawn aside by the large ones the space of between one-seventh and two-thirds of an inch; the distance of each ball from the middle of their connecting rod, being a little more than thirty-six inches. The first or immediate small results of the two experiments, thus appearing so far to be about equally favourable, it will be necessary to examine the circumstances of each of them separately, that we may be able to judge more particularly of their merits; and, first, of the Schehallien experiment.

This experiment, it is well known, was conducted by the late Astronomer Royal, Dr. MASKELYNE, than whom a more correct, faithful, and experienced individual probably never existed. The account of his measures and observations, taken in conducting it, is minutely detailed in the Philosophical Transactions of the year 1775, or in my edition of the Transactions, vol. xiii, page 702; where all the instruments and operations are particularly described, in the most plain and satisfactory manner. The principal instrument was the ten foot zenith sector; with which the meridian zenith distances of forty-three stars by three-hundred and thirty-seven observations, were carefully taken, both on the north and south sides of the mountain. The medium of all these, with

other necessary measures, gave a final result of 11.6 seconds, for the sum of the deviations of the plumb line, on both sides of the mountain; and that, in all probability, within much less than half a second of the truth. Other instruments used, were the Royal Society's transit instrument made by Mr. Bird, and an astronomical clock by Shelton, which had both been provided on occasion of the observations on the transit of Venus, in 1761 or 1769. Besides these and several other instruments, one of Ramsden's best theodolites was used, in measuring the figure and dimensions of the mountain, which was performed in the most correct manner by skilful surveyors; so as that thence an exact model of it might be made, or all its dimensions accurately taken, for computing the attraction.

By only reading over the accounts of these operations, (in the places before mentioned) made by means of such instruments, and in such hands, every person must be convinced of the impossibility that any error could have been committed, capable of causing any sensible inaccuracy in the conclusion of the work.

It remains now to describe the share which I bore in this important business; which consisted in taking all the measurements as above described, and from those data, calculating what must have been the exact magnitude of the mountain; what its attraction on the plummet, relatively to that of the globe of the earth on the same; and what must be the mean density of the earth. These computations, which employed my daily and assiduous labours during the greater part of two years, are recorded in the Philosophical Transactions of the year 1778, and also in the second volume

of my Mathematical Tracts. It may there be seen, that after computing, trigonometrically, the bearing and distance of every point in the numerous sections of the mountain, from the two observatories, I conceived it to be divided into nearly one thousand vertical columns, of given bases and altitudes. I then computed the quantity of the attraction of all these columns, on the plummet, in the direction of the meridian, when placed at the two observatories, on both sides of the hill, where the whole effect had been observed, which attraction was thus found to be expressed by the number $8811\frac{2}{3}$. I then computed, from the magnitude of the earth, what must be its attraction on the same plummet, and found it expressed by the number 87522720.

Consequently, the whole attraction of the earth, is to the sum of the two contrary attractions of the mountain, as the number 87522720 to $8811\frac{2}{3}$; that is, as 9933 to 1 very nearly; on supposition that the density of the matter in the hill, is equal to the mean density of that in the earth.

But Dr. MASKELYNE found by his observations, that the sum of the deviations of the plumb line, produced by the two contrary attractions, was 11.6 seconds. Hence then it is inferred, that the attraction of the earth, is actually to the sum of the attractions of the hill, nearly as radius to the tangent of 11.6 seconds, that is, as 1 to .000056239, or as 17781 to 1; or as 17804 to 1 nearly, after allowing for the centrifugal force arising from the rotation of the earth about its axis.

Having now obtained the two results, namely, that which arises from the actual observations, and that due to the computation on the supposition of an equal density in the two

bodies, the two ratios compared, must give the ratio of their densities, and which is therefore that of 17804 to 9933, or 1434 to 800 nearly, or almost as 9 to 5; and so much does the mean density of the earth exceed that of the hill. Consequently, if we know the density of the latter, we shall thence obtain that of the former.

At the time when this computation was first printed, in the year 1778, the real density of the hill was unknown. It was only known that it consisted chiefly of very hard and dense rocks, much heavier than common stone, which is allowed to be $2\frac{1}{2}$ times the density of water. I then, by way of example in applying the density, multiplied $\frac{9}{5}$ by $2\frac{1}{2}$, which produced $\frac{9}{2}$ or $4\frac{1}{2}$ for the density of the earth, on the smallest assumption; till such time as we should come to know more nearly what the real density of those rocks is: and therefore I must feel reason to complain, that this number ($4\frac{1}{2}$) has often been stated, rather unfairly, as my final conclusion for the earth's mean density; instead of being only the very lowest limit that might be used, till we could better learn something on that point with more certainty. But a lithological survey of the mountain being afterwards accurately made, at my earnest request, by that excellent philosopher and geologist, Mr. Playfair, the result of which was published in the Philosophical Transactions for the year 1811; I then applied his mean statement of the rocks to my own calculations, which gave me the number 5 for the density of the earth; as I published in the fourteenth volume of my edition of the Philosophical Transactions, and in the second volume of my Tracts.

In Mr. Playfair's account of the mountain, are given the

names and nature of the several rocks that compose it, with tables or lists of their densities or specific gravities. In one table is a list of thirteen specimens of densities, contained between the numbers 2.6109 and 2.6656, the medium of the whole being 2.639876. In another table, of fifteen specimens, the densities are limited between 2.71845 and 3.0642, the medium of all which is 2.81039. And the mean between these two means, gives 2.725 for the medium density of the whole mountain, admitting it to be quite solid, or without vacuities, as it appears to be on the exterior surface at least. But in the calculation in my Tracts I went even a little higher, using the number 2.75 or $2\frac{3}{4}$, thus $\frac{9}{5} \times 2\frac{3}{4}$, which gives $\frac{99}{20}$ or 4.95 for the mean density of the earth. Or, if we assume the density of the mountain still higher, as 2.8 instead of 2.75, we then obtain $\frac{9}{5} \times 2.8 = 5.05$, a little more than 5 for the earth's density; which last number 5, I therefore fix upon, in conclusion, as probably the nearest to the truth; or at least it is sufficiently large, as it is grounded on several assumptions that are most favourable for the highest result; namely, 2.777714, or $2\frac{7}{9}$ for the density of the mountain; also $\frac{9}{5}$ as rather above the calculated ratio of the densities of the earth and mountain; and lastly, the assumption of the mountain being quite solid; though it is probable that there may be cavities in most mountains, as they are generally the production either of volcanoes, or of earthquakes.

For all these reasons, then, it is highly probable, that the earth's mean density is very near five times the density of water; but not higher. If any person should still hesitate to adopt this conclusion, his hesitation must arise from doubts either on the data obtained by the measurements, or on the

accuracy of the computations made from them. But if any such person attentively read over Dr. MASKELYNE's account of the measurements, in the Philosophical Transactions of 1775, his doubts must be soon removed, as to the data supplied by the survey of the hill, or by the astronomical observations. And as to the accuracy of my own computations, made from those data, they are fully and fairly before the public, in the works before mentioned; and let any person, who doubts, look over and repeat the calculations there stated, and try if he can find any inaccuracy in them. The only possible ground of doubt in the measured data, must be in the observed deviation in the plumb line, taken by Dr. MASKELYNE; but when we consider the accuracy of the observer, and of the instruments, and read the account of the use of them, it must be then very difficult to doubt of their accuracy. On this point it is commonly acknowledged that a good observer, with the best instruments, can observe angles to a small fraction of a second. Dr. MASKELYNE's observations give 11.6 seconds for the sum of the deviations of the plumb line, from a medium of between 300 and 400 observations. Now let us suppose it possible to have committed an error of four tenths of a second in this number, and that the true number should have been 12 seconds, instead of 11.6, being an error of the twenty-ninth part of the whole. This then would cause an error of the 29th part of the result; which would reduce the density 5 to about 4.8; showing that the number 11.6 is not too small, but may be the contrary. Next, let us assume 11 seconds only, omitting the six-tenths, being almost the twentieth part of the whole, and which therefore would give nearly 5.25 for the earth's density, being still far below the

number 5.48, as deduced from Mr. CAVENDISH's experiment. Hence it appears, that our result cannot be made to agree with that of Mr. CAVENDISH, unless our 11.6 seconds be diminished to about 10.5 or 10.4, on the supposition of an error of more than a whole second in excess, in the number 11.6 seconds; which cannot be admitted, without doing great violence to the observations.

Having thus failed in our endeavour to discover any error, or even suspicion of error, in the conduct or result of the Schehallien experiment, let us now turn our attention to the other experiment, as performed by Mr. CAVENDISH. And here I must at once disclaim all expectation of meeting any failing with regard to the operator himself, whom I well knew to be a most excellent philosopher and mathematician, as well as a patient, accurate, and acute experimenter. The failure then, if any, must be expected from the nature of the machine, and of the calculations.

From the perusal of Mr. CAVENDISH's account of the machine he employed (in the Philosophical Transactions of 1778, or vol. xviii. of my edition), and the nature of the arithmetical calculations, they at once appear to be formidable and discouraging in the highest degree. The machine is small, comparatively with those in the former, or mountain experiment. It is not easily to be understood, without actually seeing it, though assisted with the view of the drawing of the whole, on account of the intricacy and perplexity of the construction. In the first place, at each end of a light wooden rod, of near two yards in length, is attached a small leaden ball of two inches diameter; the middle of the rod being fixed to and suspended by a long and very slender

copper wire; by any small movement of these balls and the connecting rod, in a horizontal direction, by the torsion or twisting of the wire, a very minute and slow vibratory motion is commenced. To produce this small motion in the two little balls, and their connecting rod, two other large balls of ten inches diameter, are connected together by certain machinery, at like distance as the former, and capable of being moved to different distances on the horizontal level with the small balls. By so setting the large balls near the small ones, these are attracted by the former, producing a very small motion in them, and in consequence a very slow vibration. So minute are these motions, that the extent of the vibrations is but a small fraction of an inch, and the duration of each vibration is not performed but in the time of several minutes, from three or four to near fifteen minutes. So minute are these motions, that telescopes and other means are necessary to view and to estimate their quantity and durations. To produce these minute motions, very complex machinery are necessarily employed, while the delicate movements are watched for many hours together, during many days, and recorded with regard to the extent and time of each vibration. Then, from these spaces and times, the density of the earth is to be calculated, by peculiar theorems, as compared with the vibrations of common pendulums that are produced by the attractions of the earth.

All these effects were so minute, and produced by machinery so complex, and the results calculated by theorems derived from intricate mathematical investigations, that it is impossible, at first, for ordinary readers to conceive how any accurate results can be deduced from them; and even for

the more judicious reader to place confidence in them, except chiefly on account of the high character of the experimenter himself. From the nature of the machinery, I could therefore derive no confidence in the results, nor compare them with the mountain experiment, without repeating the whole of the calculations. But, after a long life spent in almost daily abstruse investigations, from the tenth year of my age, and now being at eighty-four, and oppressed with distressing illness, I thought I might be excused from such a task. But, after urging more than one mathematical friend, without being able to interest them sufficiently to engage in so severe an operation, my anxiety to accomplish the business induced me to make an exertion to effect it myself; especially as the learned experimenter informs us, that he availed himself of the assistance of the then clerk of the Society, who he says made some of the experiments, and who doubtless made most of the arithmetical computations: operations, of both kinds, in which I remember he was much employed by Sir CHARLES BLAGDEN, and other gentlemen, in preparing their papers for the Royal Society. I have therefore recomputed all the experiments, and have traced the investigations of all the theorems; and have found that my labour has not been in vain; but, on the contrary, has been rewarded with the following copious list of errata, some of which are large or important.

In the following instances it is to be noted, that the references are made to Mr. CAVENDISH's paper, as printed in my edition of the Philosophical Transactions, as I am not now possessed of a set of the original edition; but with which, however, I have had my own set compared and verified.

Some of the errata in Mr. CAVENDISH'S *paper.*

In page 399, line 10 from the bottom, for 8739000, read 8740000.

In page 399, line 6 b, or from the bottom, for 8739000, read 8740000.

The same also in line 5 b.

In page 399, line 4 b, for 10683 read 10685.

The same also in line 1 b.

In page 403, lines 12 and 13, for 8739000, read 8740000.

——————— line 13, for 10844, read 10847.

———————————for 10683, read 10685.

In page 404, line 11, for 185, read 186.5.

———————lines 15, 16, 22, 25, for 185, read 186.5.

It is to be noted, that after the experiments have been all made, and the motion of the arm carrying the small balls, and expressed in twentieths of an inch, observed and denoted by the letter B; also the time of one vibration, expressed in seconds, denoted by the letter N; and both of these being corrected according to certain rules there given, then the mean density of the earth D, in each experiment, is to be computed by this theorem,

viz. $D = \frac{N^2}{10683\,B}$, or when corrected, $D = \frac{N^2}{10685\,B}$.

And by this theorem were calculated the following twenty-nine experiments, as they stand recorded in the original.

Table of the results of the experiments.

Experiments.	Motion of the arm.	The same corrected.	Time of vibration.	Ditto corrected.	The Density.
	20ths. Inc.	20ths. Inc.	M. S.		
1	14.32	13.42			5.50
2	14. 1	13.17	14 55		5.61
3	15.87	14.69			4.88
4	15.45	14.14	14 42		5.07
5	15.22	13.56	14 39		5.26
6	14. 5	13.28	14 54		5.55
7	3. 1	2.95		6 54	5.36
8	6.18	.	7 1		5.29
9	5.92	.	7 3		5.58
10	5. 9	.	7 5		5.65
11	5.98	.	7 5		5.57
12	3.03	2.9			5.53
13	5. 9	5.71	7 4		5.62
14	3.15	3.03	By	6 57	5.29
15	6. 1	5.9			5.44
16	3.13	3.00	mean.		5.34
17	5.72	5.54			5.79
18	6.32	.	6 58		5.10
19	6.15	.	6 59		5.27
20	6.07	.	7 1		5.39
21	6.09	.	7 3		5.42
22	6.12	.	7 6		5.47
23	5.97	.	7 7		5.63
24	6.27	.	7 6		5.34
25	6.13	.	7 6		5.46
26	6.34	.	7 7		5.30
27	6. 1	.	7 16		5.75
28	5.78	.	7 2		5.68
29	5.64	.	7 3		5.85

The last column shows the numbers for the required density, resulting from the calculation by the foregoing theorem,

being all a little above 5, excepting the third number, which is a little below 5. And immediately after, is the following remark, showing the author's doubt of their accuracy; *viz.* "From this table it appears, that though the experiments "agree pretty well together, yet the difference between them, "both in the quantity of motion of the arm, and in the time "of vibration, is greater than can proceed merely from the "error of observation. As to the difference in the motion of "the arm, it may very well be accounted for, from the cur-"rent of air produced by the difference of temperature; but "whether this can account for the difference in the time of "vibration, is doubtful. If the current of air was regular, "and of the same swiftness in all parts of the vibration of "the ball, I think it could not; but as there will most likely "be much irregularity in the current, it may very likely be "sufficient to account for the difference." It then proceeds: "By a mean of the experiments made with the wire first "used," (*viz.* the first six numbers or experiments) "the "density of the earth comes out 5.48 times greater than that "of water; and by the mean of those made with the latter "wire, it comes out the same; &c."

Now, though the former list of errata were but small in quantity, yet here is one of considerable magnitude, *viz.* in the medium of the first six experiments, said to be 5.48, which is very erroneous, the true medium being only 5.31; and it is rather curious that that medium 5.48 has been obtained, by taking the third experiment as 5.88 instead of 4.88, through mere oversight or carelessness. If this were the only error, it might perhaps be excused as a single accident; but the whole will make a very different appearance, when we have shown that many small errors exist in almost all the numbers

in the last column of the table, as resulting from erroneous calculations, in the use of the general theorem before mentioned, and evinced by a comparison of the numbers in the foregoing table, with those of the following one, derived by our calculation from the same data, and by the same theorem.

The corrected table of the experiment results.

Experiments.	Motion of arm corrected.	Time of vibr corrected.	Ditto in seconds.	Densities corrected.
	20ths Inch.	Min. Sec.	Seconds.	
1	13.46	14 55	895	5.49
2	13.21	14 55	895	5.59
3	15.17	14 55	895	4.86
4	14.68	14 42	882	4.89
5	14.46	14 39	879	4.93
6	13.63	14 54	894	5.41
7	2.92	6 54	414	5.41
8	3.09	7 1	421	5.29
9	2.96	7 3	423	5.57
10	2.95	7 5	425	5.64
11	2.99	7 5	425	5.57
12	2.85	6 57	417	5.62
13	2.86	6 57	417	5.61
14	2.97	6 57	417	5.40
15	2.95	6 57	417	5.43
16	2.97	6 57	417	5.40
17	2.77	6 57	417	5.79
18	3.16	6 58	418	5.10
19	3.08	6 59	419	5.26
20	3.03	7 1	421	5.38
21	3.05	7 3	423	5.42
22	3.06	7 6	426	5.47
23	2.99	7 7	427	5.64
24	3.14	7 6	426	5.34
25	3.07	7 6	426	5.46
26	3.17	7 7	427	5.30
27	3.05	7 16	422	5.38
28	2.89	7 2	422	5.68
29	2.82	7 3	423	5.85

Here the medium of the first six of these experiments is 5.19; of the other twenty-three experiments it is 5.43; and the mean of both these means is 5.31, instead of 5.48, as stated in the former table, being the error arising from the sum of the numerical calculations. The remaining difference, 0.31, about the 17th part of the whole, must therefore be ascribed to the inaccuracy of making and reading off experiments, with such intricate and inadequate machinery.

I cannot conclude this paper of enquiry, without expressing a hearty wish for the repetition of the large or mountain experiment, in some other favourable situation, and with improved means, if possible. For this purpose, I shall venture just to mention an idea which has sometimes occurred to my mind, namely, that one of the large pyramids in Egypt might profitably be employed, instead of a mountain, for this experiment. Such a body offers several advantages for the purpose. In the first place, the mass is sufficiently large, standing on a base of about the size of the whole space of Lincoln's Inn Fields, and of a height almost double of that of St. Paul's steeple; then the station for the plummet, or zenith sector, could be taken much nearer the centre of the mass, than on a mountain, which would give a larger quantity of deviation of the plummet; then the regular figure and the known composition of the mass would yield great facilities in the calculation of its attraction; lastly, the deviation of the plummet might be observed on all the four sides. Should such a project take place, it will be best to take the stations at about one fourth of its altitude above the base, that being the place where the deviation of the plummet would be the greatest. Finally, so favourable for such an experiment do

those circumstances appear, and so anxious are my wishes for its completion and success, that, were it not for my great age and little health, I should be glad to make one in any party to undertake such an expedition.

CHARLES HUTTON.

Bedford Row,
17*th March,* 1821.

XX. *On the separation of Iron from other metals. By* J. F. W. Herschel, *Esq. F. R. S.*

Read April 5, 1821.

An easy and exact method of separating iron from the other metals with which it may happen to be mixed, has always been a desideratum in chemistry. Every one conversant with the analysis of minerals is aware of the difficulty of the problem, which indeed is such that, in experiments conducted on any thing like a large scale, it might hitherto be regarded as insuperable. In consequence of this, and of the importance of the enquiry, there is hardly a chemist of eminence who has not proposed some process for the purpose, but (with the exception of that which depends on the insolubility of the persuccinate of the obnoxious metal, which I have not tried, and which is too expensive to be resorted to for any but the nicer purposes of analytical research) they are all of them either inadequate to the end proposed, intolerably tedious, or limited in their application. That which I have now to propose, on the other hand, is liable to none of these objections, being *mathematically* rigorous, of general application, and possessing in the highest degree the advantages of facility, celerity, and cheapness. It is briefly this:

The solution containing iron, is to be brought to the maximum of oxidation, which can be communicated to it by boiling with nitric acid. It is then to be just neutralized *while in a state of ebullition,* by carbonate of ammonia. The

whole of the iron to the last atom, is precipitated, and the whole of the other metals present (which I suppose to be manganese, cerium, nickel, and cobalt), remains in solution.

The precautions necessary to ensure success in this process are few and simple. In the first place, the solution must contain no oxide of manganese or cerium above the first degree of oxidation, otherwise it will be separated with the iron. It is scarcely probable in ordinary cases that any such should be present, the protoxides only of these metals forming salts of any stability; but should they be suspected, a short ebullition with a little sugar will reduce them to the minimum. If nitric acid be now added, the iron alone is peroxidized, the other oxides remaining at the minimum.* Moreover, in performing the precipitation the metallic solution should not be too concentrated, and must be agitated the whole time, especially towards the end of the process; and when the acid reaction is so far diminished that log-wood paper is but feebly affected by it, the alkaline solution must be added cautiously, in small quantities at a time, and in a diluted state. If too much alkali be added, a drop or two of any acid will set all right again; but it should be well observed, as upon this the whole rigour of the process depends, that no inconvenience can arise from slightly surpassing the point of precise neutralization, *as the newly precipitated car-*

* Dr. FORSCHAMMER, in a paper recently published in THOMSON's Annals of Philosophy, contends that the proto-salts of manganese are absolutely void of colour. To this I can only say, that I have not succeeded in depriving the muriate of its pale rose colour by any length of ebullition with sugar or alcohol, after which, however, not a trace of deutoxide could be detected in it. I cannot help regarding the process here proposed for freeing manganese from iron as preferable to that of Dr. F.

bonates of the above enumerated metals are readily soluble, to a certain extent, in the solutions in which they are formed (though perfectly neutral). In the cases of cobalt and cerium, this re-dissolution of the recent precipitate formed by carbonate of ammonia is very considerable, and a solution of either of these metals, thus impregnated with the metallic carbonate, becomes a test of the presence of peroxide of iron, of a delicacy surpassing most of the re-agents used in chemistry, the minutest trace of it being instantly thrown down by them from a boiling solution, provided no marked excess of acid be present. To be certain however that we have not gone too far, it is advisable, after separating the ferruginous precipitate, to test the clear liquid, while hot, with a drop of the alkaline carbonate. If the cloud which this produces be clearly re-dissolved on agitation, we may be sure that only iron has been separated. If otherwise, a little acid must be added, the liquor poured again through the filter, so as to wash the precipitate, and the neutralization performed anew.

The precipitation of iron above described seems at first sight to result from a double decomposition. Were it so, the principle of the method would be merely a difference of solubility in the carbonates of iron and the other metals, and as such would have no claim to be regarded as rigorous. Such however is not the case. The iron is not separated in the state of a carbonate, but of a sub-salt, or a simple peroxide, the whole of the carbonic acid escaping with effervescence at each addition of the alkali. The phænomenon turns on a peculiarity in the peroxide of this metal, in virtue of which it is incapable of existing in a neutral solution at the boiling

temperature. If we add an alkaline, earthy, or metallic carbonate by little and little to a *cold* solution of peroxide of iron, the precipitate formed is re-dissolved with effervescence, readily at first, but gradually more and more slowly, till at length many hours, or even days, elapse before the liquid becomes quite clear. Meanwhile it deepens in colour till (unless much diluted) it becomes dark brown or red. If the addition of the carbonate be carried as far as possible without producing a permanent precipitate, the solution is perfectly neutral, and continues clear at a low temperature for any length of time. In this state it may be evaporated to dryness in *vacuo,* and the residue (which *does not effervesce* with acids) is still soluble in water without letting any iron fall, and so on as often as we please.

The compound thus formed is however far from permanent. It is in fact in a state of tottering equilibrium, which a very slight cause is sufficient to overset. Supposing the point of saturation to have been exactly attained, the addition of an extremely small quantity *more* of the alkaline solution is sufficient to determine the separation of the whole, or nearly the whole, metallic contents; and if the solution operated on be pretty concentrated, it fixes after a longer or shorter time into a stiff and almost solid coagulum. Again, if to the coagulum so formed, a quantity equally inappreciable of the original ferruginous solution be added, it gradually liquefies, and after some time is completely re-dissolved (forming no inapt representation of the celebrated imposture of St. Januarius's blood)*

* The phænomenon described in the text appears to me to differ from ordinary precipitations and solutions, in the small proportion between the precipitant and the

A similar change is produced by an increase of temperature. If we heat a solution exactly neutralized as above described, it speedily grows turbid, deposits its ferruginous contents in abundance, *and at the same time acquires a very decided acid reaction.* The acid so developed holds in solution a portion of oxide, but if the neutralization be performed afresh *while hot*, this separates entirely, and the liquid after filtration has no more action on gallic acid, ferrocyanate, or sulphocyanate of potash, than so much distilled water.*

It is not my object in this paper to enter into any minute detail of the nature of the persalts of iron, a subject not nearly exhausted, and which want of leisure alone has prevented my entering upon, but merely to point out the practical application of this one of their properties, to an important

precipitate, the solvent and the matter dissolved. I can call to mind but one instance of so small a quantity of matter operating a chemical change on so large a mass, *viz:* the decomposition of oxygenated water by fibrin and other animal substances. The action seems to be propagated from particle to particle. Whether the superabundant oxide of iron be retained in solution in a state at all analogous to that of the oxygen in Thenard's experiments, might possibly deserve consideration.

* It was in 1815, in the analysis of a specimen of the gold ore of Bakebanya, given me for that purpose by Dr. Clarke, that I first remarked the separation of oxide of iron from a clear neutral solution by mere elevation of temperature, and attributed it to the presence of an oxycarbonate capable of subsisting in a low temperature, but decomposed by heat. That this is not the true explanation is already shown, and I have considerable doubt of the existence of a percarbonate of iron at *any temperature.*

The most elegant mode of exhibiting the experiment is perhaps the following: having rendered a solution of proto-sulphate of iron rigorously neutral, by agitation with carbonate of lime and filtration, dissolve in it a small quantity of chlorate of potash (a salt perfectly neutral). The solution when raised to ebullition is peroxidized, a quantity of sub-sulphate precipitates, and the supernatant liquid is found decidedly, and even strongly acid.

object in analysis. The principle here developed furnishes a ready method of detecting the minutest quantities of other metals in union with iron, and therefore cannot but prove of important service in various cases where this metal constitutes the chief ingredient in the substance examined, as in meteoric iron, the various natural oxides of this metal, &c. &c. I will exemplify this in one or two instances.

36·00 grains of meteoric iron (furnished me by the kindness of Dr. Wollaston) were dissolved in dilute nitro-sulphuric acid, leaving behind a minute quantity of a brilliant black powder, which however dissolved by digestion in nitro-muriatic acid, and appeared only to contain an excess of nickel. The solutions were mixed, and being neutralized at a boiling temperature by carbonate of ammonia, and the iron separated, a green solution remained. Into this when boiling, a drop of persulphate of iron being let fall, was immediately precipitated in the state of subsulphate, which being separated, the solution was boiled with excess of caustic potash till all smell of ammonia disappeared. Oxide of nickel separated, which collected and strongly ignited, weighed 4·65 grains, or 12·92 on the hundred, which (taking the atom of nickel to weigh 30, and that of oxygen 8, hydrogen being unity) gives 10·20 per cent for the contents of the specimen analyzed in metallic nickel.

100 grains of titanious iron from North America, being dissolved in muriatic acid (after the requisite ignition with potash) were treated (after separating the titanium) with excess of carbonate of lime and filtered. The excess of carbonic acid being expelled, ammonia was added, and a small quantity of a white precipitate fell, which speedily blackened

in the air, and proved to be mere oxide of manganese, uncontaminated by iron, and amounting to half a grain.

Manganese has been suspected in various species of cast iron; and though Mr. MUSHET's experiments go to prove that it does not usually enter in abundance, they can hardly be regarded as establishing the fact of its absence. It might not be uninteresting to resume the investigation with the aid of a mode of analysis so well adapted to experiments on a large scale, as I have no doubt that, with proper care, one part in a thousand, or even less, of manganese might be insulated from iron.

The separation of iron from uranium cannot be accomplished by the process above described, that metal possessing a property analogous to that which forms the subject of this paper. By inverting the process, however, we shall succeed even here. A mixed solution of iron and uranium being de-oxidized by a current of sulphuretted hydrogen, and then treated with an earthy carbonate, the iron passes in solution while the uranium separates. This difference in the habitudes of the two oxides of iron presents us in fact with a kind of chemical dilemma, of one or the other of whose horns we may avail ourselves in any proposed case. In studying the habitudes of uranium, however, I have met with some anomalies which require farther investigation. Zirconia too might probably be freed from iron with equal facility by a similar inversion of the process; but this I have not yet had an opportunity of trying satisfactorily.

J. F. W. HERSCHEL.

London,
April 4, 1821.

XXI. *On the re-establishment of a canal in the place of a portion of the Urethra which had been destroyed. By* HENRY EARLE, *Esq. Surgeon to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital. Communicated by Sir* HUMPHRY DAVY, *Bart. P. R. S.*

Read April 12, 1821.

IF any apology be requisite for bringing forward the following insulated fact, I hope it will be found in its tendency to throw some light on an interesting physiological subject, which has lately occupied the attention of this learned Society, as well as in its novelty and general importance.

Of all the complaints to which the human body is liable, there is, perhaps, no class more productive of corporal and mental suffering, than the various affections of the male urethra. For it is most obvious, that any deviation from healthy structure in that part, which, from its peculiar function, is called into action after very short intervals of repose, must, from that circumstance alone, be productive of almost constant suffering; while the mind of the patient is also depressed from the effect of a continual anticipation of pain, and the apprehension of impaired virility.

The following is a statement of the result of a new operation in a very aggravated case, by which an individual has been raised from such a state of despondency to one of comparative happiness.

John Whitaker, whilst serving on board his Majesty's ship Pylades, off the island of Sardinia, in May 1813, when returning on board from Magdalina, fell with one leg on each side of the boat, the stem of which injured his urethra so much in the perineum, that he was obliged to have the catheter introduced for above six weeks. From that time he continued to experience more or less difficulty in discharging the contents of the bladder until the beginning of May, 1819, when he was attacked with a sudden retention of urine, which was soon followed by extensive effusion into the cellular substance. Before he could obtain surgical assistance mortification had taken place, and the integuments in the perineum, with above an inch of the canal of the urethra, had sloughed away. A free vent being thus obtained, the mischief did not extend itself to the scrotum. During the healing process, the medical gentleman who attended him, made several unsuccessful attempts to unite the integuments over a catheter.

He came under my care the following August, in Saint Bartholomew's Hospital; at which time a large smooth cicatrix occupied the place of the urethra, no vestige of which remained at that part. The mucous membrane of the canal was distinctly visible, terminating above, and recommencing below, the cicatrix. Through the posterior aperture the whole of the urinal and seminal discharges came away, while the anterior portion of the urethra, particularly that part which passed behind the scrotum, was encreased in density and much contracted, and probably would ultimately have been completely obliterated by disuse.

The man was by trade a carpenter; and, as he was obliged to work, it was a very serious inconvenience to him every

time he obeyed the calls of nature. This, coupled with the distressing excoriation attendant on the scattering of the urine, made him anxious to submit to any plan of treatment which afforded a possibility of relief, and I determined on pursuing the following one.

The integuments on the right side had suffered less extensively than those on the left, so that when a catheter was introduced, that portion which passed across the cicatrix could be about half covered by drawing the skin and cicatrix from the right towards the opposite side. My first attempt, therefore, was to encourage this disposition in the integuments to fold over; and as some delay was requisite in order to dilate the anterior part of the urethra with bougies, he was directed to remain in bed with his knees tied together over a pillow, and a truss was so applied as constantly to press the integuments from the right to the left side.* To this plan the ultimate cure of the patient is in some measure referable.

After some weeks, the urethra being sufficiently dilated to admit a moderate sized catheter, I determined to attempt the following operation. The smooth cicatrized surface having become insensible to the irritation of the urine, I resolved to employ it in the formation of a canal, and to endeavour to connect by it the two portions of the urethra: for, as many

* I constructed a similar truss some years before, for the relief of a female suffering under incontinence of urine, and have since successfully employed it in three cases. It consisted in a spring which passed round the front of the body above the pubes, and fastened with a strap behind. From the centre of the truss a fine spring descended, taking the necessary curve to pass under the arch of the pubes, and terminating in an oval pad covered with oil silk, about an inch in length, and half an inch wide.

months had elapsed since the healing of the wound, all contraction in the cicatrix had ceased, and it was probable that a passage formed of such parts would not be liable to any farther diminution in its calibre.

On the other hand, I had to contend with two great difficulties: in the first place, the portion of cicatrized integument intended to be separated, was not of original formation; consequently, it was endued with less vital energy, and possessed fewer blood vessels: secondly, it was not possible to allow the parts to be at rest for the completion of any curative process for many hours together; the force also with which the urine was expelled, and the acrid nature of that discharge, were alike unfavourable to the cure by adhesive inflammation. All these circumstances having been well considered, a portion of integument was removed about an inch and half long, and one-third of an inch in width, on the left side of the cicatrix; the groove thus formed being intended to receive the edge of skin to be detached from the opposite side. An incision was then made across the perineum above and below, so as to pare away the callous edges of the urethra. The cutis was next dissected off from a portion of integument on the right side of the perineum, about an inch and half in length and half an inch broad, leaving a smooth space of rather more than an inch between the cut surfaces, which was intended to form the lining of the new canal. The integuments on the right side were now dissected up, turned over a catheter, and brought in contact with the opposite groove. The detached portion of cicatrix bled little during the operation, and, before it could be applied to the groove, the edge had so livid an appearance as to create an apprehension that

it must perish. Two ligatures were employed to assist in retaining it in the desired position, and some straps of adhesive plaster and a bandage completed the dressings. The day following the operation, it was evident that some urine had escaped by the side of the catheter; and on the third day, when it became necessary to remove the dressings, it was found that the portion of the flesh which had been denuded of skin had sloughed, but that a sufficient quantity had united above and below to form a canal open at one side, and large enough to include the whole catheter.

This result was quite as favourable as could, under all circumstances, have been expected; and I was led to entertain sanguine expectations of ultimate success. The two surfaces, from whence the integuments had been removed, were now suffered to heal; but as the cicatrix on the right side contracted, it drew the newly formed canal rather to that side, and tended to increase the opening into it. It was, consequently, determined not to attempt any thing farther until all contraction had ceased. So much, however, had been gained by this operation, that when the catheter was introduced, and the finger pressed on the left side, no urine escaped, and some could be made to pass through the penis without the aid of the catheter. My patient however, soon after this, became much disordered in his health, and had an attack of lepra vulgaris, to which he had for years been subject, on which account for some months nothing was attempted, except several times freely excoriating the edges of the canal, and thus endeavouring to unite them by keeping them in contact. In this we were constantly foiled by the astonishing rapidity with which the skinning process took place from

within outwards. This disposition to form new skin was so remarkable, as to excite the surprize of several gentlemen who witnessed it, and appeared to arise from the moist state in which the parts were constantly kept.*

In the summer of 1820, the man had recovered from his cutaneous affection, and his general health was so much improved, that he resolved to submit to a second operation. In this attempt I borrowed integuments from the opposite side to that I had taken them from in my first. A deep groove was made on the right side, the surface was denuded of its cutis to some extent, a considerable portion of integument was then detached from the left side, and, in order to obtain healthy skin, I encroached a little on the thigh, and laid bare the edge of the fascia lata. Instead of passing any ligature through the detached portion, the old quill suture was employed, which was passed from the two outer cut surfaces. A pad of adhesive plaster was interposed between the ligatures and the flap of skin, to diffuse the pressure more generally; and my patient, being now quite an adept in passing the catheter, was directed to introduce it about three times in the twenty-four hours, instead of retaining it in the bladder,

* In corroboration of this, I have lately employed bread and water poultices to healthy sores, which have skinned over with greater rapidity than under any other application. Since making these experiments, I have learnt that Professor KERN, of Vienna, employs no other local remedy in the cure of ulcers, than water and a simple covering of linen. It is a curious fact, that in the sixteenth century, when the art of surgery was encumbered with useless nostrums and complicated instruments, and when the actual cautery and hot oils were the favourite remedies, that a similar simplicity of treatment should have been employed by MAISTRE DOUBLET, a contemporary of AMBROSE PAREY, of whom BRANTOME tells us,

"Et toutes ses cures faisoit le dit DOUBLET par un simple linge blanc et belle eau simple de la fontaine ou des puits."

which had permitted some of the urine to pass insensibly away, and had acted prejudicially in the former operation. By this attempt much more was gained, and about two-thirds of the canal were completed; still, however, there remained a small aperture at the upper part. We again attempted to close this by denuding the edges with escarotics and the lancet, but it skinned over too rapidly to allow of any union between the opposite surfaces. A third operation on a smaller scale was therefore necessary, which so nearly completed the cure as to leave only an orifice large enough to admit a bristle, which has subsequently closed, and, at the present time (March, 1821), he remains perfectly well, and is able to expel the contents of his bladder *pleno rivo.*

I may perhaps incur the charge of prolixity in the foregoing narrative, but I conceive it important to give a circumstantial account of the whole process, to mention all the difficulties I had to contend with, and the means which were employed to surmount them. Should I ever have occasion to repeat this operation, I should entertain sanguine hopes of succeeding at once, by avoiding some circumstances, and availing myself of others, a knowledge of which could only be gained by actual experiment.

The above case is, I believe, the first on record in which so extensive a portion of the whole canal of the urethra has been restored; and the mode of performing the operation has never, as far as I have been able to ascertain, been resorted to before. Mr. A. Cooper, in the second part of his Surgical Essays, which was published soon after my first operation on Whitaker, has given an account of two very interesting cases, in which he succeeded in closing unnatural openings

in the urethra. In neither, however, of the cases which he has related, was the breach so extensive, nor did it occupy the whole canal. The second case, related in page 207, approaches nearer to WHITAKER's than the first. In this instance the opening was anterior, but close to the scrotum; and Mr. COOPER availed himself of this circumstance in effecting a cure; a portion of the skin of the scrotum was partially detached, and turned over so as to cover the opening, the callous edges of which were previously pared away. The operation in this case differed materially from the one which I performed; for in Mr. COOPER's case, the raw surface was turned towards the urethra, whilst in mine the canal was wholly formed of a previously cicatrized smooth surface, which had undergone its utmost degree of contraction before it was employed to form the canal.

The fact I conceive is new, that, from a cicatrix of common integuments, a canal may be formed capable of conveying so acrid a fluid as urine, and of fulfilling *all* the functions of a healthy urethra, without being liable to any subsequent variation in its calibre. It is important, also, in throwing some light on the still disputed question of the muscularity of the urethra; for since the patient quitted my care, he has more than once indulged in sexual intercourse; and he assures me, that the jet of semen is as forcible as before the accident. When we consider that nearly, if not entirely, the whole of the ejaculator seminis must have sloughed away with the portion of the urethra which perished, and that an interval of above an inch of common integument at present exists between the two portions of the meatus urinarius, it is difficult to account for this phenomenon. It is probable, that

the semen is in the first instance projected into the urethra with some impetus, and it would there immediately receive additional impulse from the spasmodic action of the levator ani and other muscles in the neighbourhood of the urethra; but the vis a tergo must be nearly, if not entirely, lost in its passage through the portion of integument in the perineum. It must then depend for its final projection, either on the muscular fibres which have been described by Mr. Bauer as surrounding the mucous membrane of the urethra at its anterior part, or on the elastic property which has been assigned to it. If I might venture to offer an opinion on the subject, I should consider the present case rather in favour of the muscularity of the urethra, as the quantity of fluid secreted is hardly sufficient to distend the whole canal, a circumstance very essential to the reaction of an elastic tube. From the tortuous course of the muscular fibres, as described by Mr. Bauer in Sir E. Home's paper, it seems probable that they would require to be elongated before they could act with force; and precisely such would be the effect of the injection of blood into the corpus spongiosum which takes place in coitu.

One more circumstance I may venture to allude to, as tending to support such an opinion, namely, the complete emission of the contained fluid which takes place, which requires a forcible and very sensible contraction of the whole canal, and cannot be accounted for on the supposed principle of elastic compression, unaided by muscular action.

On reflecting on the preceding case, it appears to me not less important in a practical than a physiological point of view; for the curative principles which were acted on, may

lay the foundation of an improved mode of treating some of the more lamentable cases of strictures with fistulous openings and diseased integuments in perineo. It is well known, that such cases occasionally baffle the skill of the ablest practitioners, and often terminate in premature death after years of continual suffering. When we consider that that part of the urethra situated opposite the perineum, is by much the most frequent seat of disease, and that it is often confined to this situation, it is probable that in such cases, if we could remove the diseased portion of the urethra, together with the thickened fistulous integument, much good might be effected; and perhaps even a permanent cure might be accomplished, by subsequently pursuing a somewhat analogous operation to the one performed on Whitaker. Such a practice would, I conceive, be justifiable on two grounds. In the first place, the patient's state is nearly hopeless from all common plans of treatment, and should the operation not eventually succeed, he will not be rendered worse; for instead of making water through numerous fistulous apertures, and being subject to frequent depôts of urine and the formation of fresh abscesses, he would at once empty his bladder from the extremity of the membranous part of the urethra; and farther it may be urged, that no parts of vital importance would be endangered by the operation. It is true, that such a plan would be both painful and tedious, but I should still consider it worth the experiment, after in vain trying all the usual modes of relief. The case just related, and the success which attended Mr. Cooper, encourage us to hope that, in many cases which have hitherto been abandoned as incurable, much good may yet be effected by judicious treatment, and

a right application of the known laws of the animal economy. Should future operations be equally successful, the borrowing from one part of the body to repair the loss of another, must be considered as one of the happiest modes of directing the reparative processes of nature; for the closing of large fistulous openings in the male and female urethra, must certainly be acknowledged as contributing more essentially to the happiness and comfort of an individual, than almost any other operation in surgery.

HENRY EARLE.

28, George Street, Hanover Square,
March 22, 1821.

XXII. *Calculations of some observations of the solar eclipse on the 7th of September, 1820. By* Charles Rumker. *Communicated by* Thomas Young, *M. D. For. Sec. R. S.*

Read May 10, 1821.

BE } signifies { Beginning } of the Eclipse. BR { signifies { Beginning } of the Ring.
EE } signifies { End } of the Eclipse. ER { signifies { End } of the Ring.

Place of Observation.	Latitude.	Long. in Time from Greenwich.	Observer.	Phase.	Instant of Observation in Mean Time.	Conjunction in Mean Time.	Coefficients of the Corrections.
Nienstedten . .	53 33 10	39 25 E	Schumacher	B. E.	1 10 38,5	2 29 30,5	—0,5846dL+2,2958d(R+r)+0,1994
Bremen . . .	53 4 38	35 12	Olbers	B. R.	2 29 24	2 25 21,1	+0,0403dL+2,221 d(R—r)—0,1542
				E. R.	2 34 41,5	2 24 55,5	—1,245 —2,545 d(R—r)+0,8942
				E. E	3 52 13	2 25 4,8	—0,5706 —2,292 d(R+r)+0,1201
Göttingen . . .	51 31 56	39 47	Gauss	B. R.	2 28 11,1	2 29 52,2	+0,047dL +2,2207d(R—r)—0,2386
				E. E.	4 0 39,0	2 29 32,4	—0,5731 —2,293 d(R+r)+0,0461
Berlin	52 31 15	53 31,5	Bode	E. E.	4 13 44,7	2 43 16,4	—0,6351dL—2,310 d(R+r)+0,1142
Bologna . .	44 30 12	45 26	de Zach	B. E.	1 35 31,3	2 35 30,51	—0,7876dL+2,3557d(R+r)+0,4612
				E. R.	3 5 0,32	2 35 20,42	+1,5174dL—2,6891d(R—r)—1,7192
Genoa . . .	44 24 34	35 47	Ruppel	E. E.	4 11 59	2 25 34,8	—0,4579dL—2,2669d(R+r)—0,3458
Copenhagen . .	55 40 55	50 20	Ursin	B. E.	1 21 22,2	2 40 32,4	—0,4614dL+2,2676d(R+r)+0,1300
				E. E.	4 3 22,1	2 40 11,5	—0,6500 —2,3134 +0,2675
Cuxhaven . . .	53 52 40	34 51	Tralles	B. E.	1 4 10,4	2 24 55,5	—0,6087dL+2,302 d(R+r)+0,7319
				B. R.	2 27 25	2 24 57,6	+0,3360 +2,245 d(R—r)—0,370
				E. R.	2 32 27,9	2 24 33,3	—1,628 —2,752 +1,2506
				E. E.	3 49 58,7	2 24 41,1	—0,5743 —2,293 d(R+r)+0,1585
Hamburg . . .	53 33 8	39 58	Rumker	E. E.	3 56 27,9	2 29 47,5	—0,5931dL—2,298 d(R+r)—0,1489
Manheim . . .	49 29 18	33 53	Nicolai	B. R.	2 35 25,5	2 23 49	—2,549 dL+3,380 d(R—r)+1,807
				E. R.	2 40 21,6	2 23 44,4	+0,6986 —2,3275d(R—r)—0,8319
				E. E	3 58 34,5	2 23 41,4	—0,5246 —2,280 d(R+r)—0,0687
Near Cork, 20″ East thereof in Time.	51 55 31	33 36 W	Brisbane	B. E.	11 38 30,6	1 16 21,94	—1,2222dL+2,5343d(R+r)+1,4853
				E. E.	2 32 26,1	1 16 17,27	—0,2173 —2,2308d +0,0251
Bushy Heath, Stanmore . . .	51 37 44,3	1′ 21″	Beaufoy	B. E.	0 22 57	1 48 47,69	—0,9545dL+2,4166d(R+r)+1,1170
				E. E.	3 14 57	1 48 28,55	—0,3875 —2,2538d +0,0142
Greenwich . .	51 28 40	0 0	Pond	B. E.	0 22 37	1 50 0,6	—0,9507dL+2,4151d(R+r)+1,1027.
				E. E.	3 14 40	1 49 48,8	—0,39215 —2,2555 +0,0099
Blackheath . .	51 28 2	0 0,3 E	Groombridge	E. E.	3 14 32,8	1 49 40,4	—0,3906dL—2,2547d(R+r)+1,0100
Kentish Town . .	51 33 34	0 35,2 W	F. Baily	B. E.	0 21 42,4	1 49 24,64	—0,9526dL—2,4159d(R+r)+1,1100
				E. E.	3 13 41,1	1 49 7,78	—0,3896 —2,254 +0,0107
Zurich Observatory	47 22 27	34 11 E	Freer	B. R.	2 42 15,03	2 24 17,68	Diff. of Radii 65,59. Diff. of Latitudes 6
				E. R.	2 43 49,8	2 24 14,74	+3,629 dL—4,2553 d(R—r)—3,16dp.
Zurich Town . .	. . .	. . .	Horner	B. R.	2 42 3,88	2 24 9,06	Diff. of Radii 65,59. Diff. of Latitudes 6
				E. R.	2 43 41,42	2 24 11,47	+3,846dL—4,4408d(R—r)—8,46dp.
Amsterdam . . .	52 22 17	19 33 E	Greve	B. R.	2 13 30,5	2 9 39,19	Diff. of Radii 65,58. Diff. of Latitudes 67
				E. R.	2 14 24,5	2 9 32,4	—6,1821dL—6,5686d(R—r)+5,03097
Bergen	60 23 38	21 24 E	Bohr	B. R.	1 58 53,23	2 11 17,49	+1,0562dL+2,4586d(R—r)—3,708dp
				E. R.	2 2 54,08	2 10 41,41	—2,554 —3,3841 +2,3537.

From the three following observations of Doctor OLBERS, I calculated the elements of the comet in the Pegasus 1821.

	Mean Time at Bremen.	Comets appar. AR.	Ditto, declination, North.
January 30	7° 17′ 51″	359 27 4	16 5 1
February 19	6 49 20	357 59 48	14 48 10
March 6	6 56 20	356 46 33	13 34 21

Thence I find

Transit over the perihelion, March, 21,6114625 mean time at Bremen.
Long. of the perihelion 239° 35′ 53″ upon the orbit.
Long. of the d. Node 48 44 18
Inclination of the orbit 73 20 00
Log. perihelion distance 8,9651463.
Motion retrograde.

These calculations are founded upon BURCKHARDT's lunar and CARLINI's solar tables.

From the former, the moon's place for mean noon at Paris, on the 7th of September 1820, was found.

	s.	°	′	″
Moon's apparent longitude -	5	13	49	24,13
Latitud -			49	59,46
Equation, horizontal parallax -			53	53
Moon's semidiameter - -			14	41
CARLINI's solar table give ⊙ latitude +				0,44
Horizontal parallax - - -				8,76
Semidiameter - - -			15	54,8

Ratio of the axes of the earth 302,8 : 303,8.

The above calculations resolved after the method of the least squares (Methode des moindres carrés) give for the error of BURCKHARDT's lunar table

$d.$ Lat. $= -3{,}975''$; $d(R - r) = -3{,}768$ $d(R + r) = -3{,}497$.

The semidiameter of the sun at a solar eclipse is therefore to be diminished by 3 ,632.* The semi-diameter of the moon appears not to require any correction after BURCKHARDT's tables.

These corrections being applied to the former calculations, we obtain the following results. The places of observations are ranged in the order in which they follow in longitude from east to west.

Place of observation.	Phase observed.	Conjunction corrected.
Moskwa -	Beginning of the eclipse	4 20 38,06.
Berlin - -	End of the eclipse	2 43 27,0
Copenhagen	Beginning of the eclipse End of the eclipse	2 40 26,09. 2 40 22,16.
Bologna -	Beginning of the eclipse End of the ring	2 35 25,38. 2 35 24,58.
Hamburg -	End of the eclipse	2 29 57,9.
Göttingen -	Beginning of the ring End of the eclipse	2 29 43,57. 2 29 42,73.
Nienstedten	Beginning of the eclipse	2 29 24,77.
Genoa - -	End of the eclipse	2 25 44,53.
Bremen -	Beginning of the ring End of the ring End of the eclipse	2 25 12,57. 2 25 10,04. 2 25 15,07.
Cuxhaven	Beginning of the eclipse Beginning of the ring End of the ring End of the eclipse	2 24 49,86. 2 24 47,805. 2 24 50,14. 2 24 51,39.
Zurich Observatory -	Beginning of the ring End of the ring	2 24 17,68. 2 24 16,35.

* This agrees with DU SEJOUR's allowance for irradiation; but the correction —3″,5 which he applied to the ☾'s semidiameter on account of inflexion, may have arisen from the imperfect state of the lunar tables.

Place of observation.	Phase observed.	Conjunction corrected.
Manheim -	Beginning of the ring End of the ring End of the eclipse	2 23 46,40. 2 23 50,47. 2 23 51,46.
Bergen -	Beginning of the ring End of the ring	2 11 4,03. 2 11 4,31.
Greenwich	Beginning of the eclipse End of the eclipse	1 49 55.9. 1 49 58,23.
Kentish Town	Beginning of the eclipse End of the eclipse	1 49 19,97. 1 49 17,22.
Bushey Heath	Beginning of the eclipse End of the eclipse	1 48 43,02. 1 48 38,00.
Near Cork	End of the eclipse	1 16 25,93.

Allowing the longitudes of the following places to be well ascertained, Greenwich = 0, Göttingen 39′ 47″, Manheim 33′ 53″, Bremen 35′ 12″, Copenhagen 50′ 20″, Berlin 53′, 31″, 5, this solar eclipse may serve to fix the longitudes of the other places, comparing the same phases observed on different spots with one another.

Thus I find the longitude of Cuxhaven.

Phase.	Beginning of eclipse.	Beginning of the ring.	End of the ring.	End of eclipse.
Compared with	Green. 34 53,96	Gotting. 34 51,23 Brem. 34 47,23 Manh. 34 54,40 Mean 34 50,97	Brem. 34 52,1 Manh. 34 52,7 34 52,38	Berlin 34 55,9 Gott. 34 55,66 Green. 34 53,16 Copen. 34 49,23 Manh. 34 52,93 Brem. 34 48,32 Mean 34 52,53

Hence the longitude of Cuxhaven is found 34′ 52″,46 in time, east. In the same manner the longitudes of the following places have been deduced.

	°	′	″	
Moskwa, longitude - -	2	30	42,16	East.
Bologna - - -	0	45	27,71.	
Hamburg - -		39	59,04.	
Nienstedten - - -		39	28,87.	
Genoa - - -		35	45,67.	
Cuxhaven - -		34	52,46.	
Zurich - - -		34	19,7.	
Bergen - - -		21	6,86.	
Amsterdam - - -		19	38,49.	
Kentish Town - -		0	40,82	West.
Bushey Heath - -		1	19,7.	
Cork - - -		3	52,93.	

XXIII. *An account of the re-measurement of the cube, cylinder, and sphere, used by the late Sir* George Shuckburgh Evelyn, *in his enquiries respecting a standard of weights and measures. By Captain* Henry Kater, *F. R. S.*

Read June 7, 1821.

The valuable experiments made by the late Sir George Shuckburgh Evelyn, for the determination of a standard of weights and measures, are detailed in the Philosophical Transactions for 1798. It may there be seen that a cube, a cylinder, and a sphere of brass were employed, the respective dimensions of which being given, as well as the weight of water displaced by each, the weight of a cubic inch of distilled water might thence be readily ascertained.

In reviewing these experiments, so much care appears to have been bestowed on those parts of the enquiry which relate to *weight*, as to leave no reason to doubt their accuracy; but as Sir George Shuckburgh has not entered into so full a detail of the method he pursued in the *measurement* of the cube, the cylinder, and the sphere, I felt it to be desirable that this operation should be repeated, before the Commissioners of Weights and Measures should make their final Report.

The Honourable Charles C. Jenkinson, to whom the valuable apparatus of the late Sir George Shuckburgh now belongs, very obligingly confided it to the care of the Commissioners. I found the sphere in the most perfect state of preservation. The cube and the cylinder were in some parts covered with an oxide, which was, however, readily removed without their sustaining any injury, by a very weak mixture of sulphuric acid and water.

Two small rectangular pieces of plate brass were prepared, of the same size, and about the tenth of an inch thick; one of the surfaces and one side of each were ground perfectly flat, and the surfaces being placed in contact, two fine dots were made on the plane sides, close to the edges, as in the accompanying figure. These pieces were intended to be applied to the extremities of the object to be measured, the dots serving as points, the distance between which was to be ascertained.

In order to keep the brass pieces in their proper position, and at the same time to ensure, in every case, an equal pressure, two springs were made to slide along a mahogany rule, divided into inches. These springs projected nearly at right angles from the rule, and being set at the required distance from each other, retained by their pressure the brass pieces steadily in the situation in which they were placed.

The micrometer microscope used on this occasion, differed essentially from that which is commonly employed. The microscope itself was carried along by the micrometer screw, instead of the motion being confined to its cross wires.

By this construction, which was suggested by Dr. Young, no error could arise from the image not being in the same plane with the wires; and it gave me, besides, the advantage of applying an object-glass, of whatever power I pleased, to the microscope, without altering the value of the divisions of the micrometer.

Having placed the rectangular pieces of brass, with their surfaces in contact, and the sides on which the dots were made in the same plane, they were confined in this position between the springs before described.

The following observations were then made with the

micrometer microscope to determine the distance between the dots.

Readings of the Micrometer.		Distance between the Dots.
Divisions.		Divisions.
10	314	304
14	317,5	303,5
13	316,5	303,5
4,5	309	304,5
84	388	304
85	389	304
9,5	314	304,5
	Mean	304

The value of one division of the micrometer was found by a number of trials to be ,00009758 of an inch; the distance between the dots is therefore equal to ,0296582 of an inch.

For a particular description of the cube, sphere, and cylinder, I shall beg leave to refer the reader to Sir GEORGE SHUCKBURGH'S paper in the Philosophical Transactions for 1798; and for the correction of some errors in computation, to a paper by J. FLETCHER, Esq. given in the 4th vol. of NICHOLSON'S Journal, 8vo.

The letters used in the following detail, indicate the same parts as in Sir GEORGE SHUCKBURGH'S paper. The letters and lines made by Sir GEORGE SHUCKBURGH on the cube, sphere, and cylinder, in black lead pencil, still remained, and afforded the means of examining each step in succession.

Great care was taken by leaving the apparatus together more than 24 hours, and by other precautions, to guard against errors which might arise from difference of temperature; and the same portions of Sir GEORGE SHUCKBURGH'S standard scale were used, as were employed by himself.

The microscopes were attached to a strong frame of well seasoned wood, and were transferred from the dots to the scale at each reading.

Measurement of the cube.

The brass pieces being properly placed, the excess of the distance between the dots and 5 inches, taken upon the standard scale between 27 and 32, was measured by the micrometer. This being added to 5 inches, and ,0296582 subtracted, we have the length of the side of the cube.

Cube.

Reading of the Micrometer at the Scale.	Reading of the Micrometer at the Dots.	Difference.	Distance between the Dots in Inches.	Length of the side of the Cube in Inches.	Mean.
Side 1. (the top.)					
22	*a* to *b* 217	195	5,0190242	4,9893660	4,98935
22	*a* to *c* 217	195	5,0190242	4,9893660	
22	*c* to *d* 220	198	5,0193169	4,9897687	
22	*b* to *d* 212	190	5,0185364	4,9888782	
Side 2. (the bottom.)					
22	*a* to *b* 220,6	198,6	5,0193754	4,9897172	4,98935
23	*a* to *c* 221	198	5,0193169	4,9896587	
22	*c* to *d* 215	193	5,0188291	4,9891709	
17,8	*b* to *d* 207,5	189,7	5,0185071	4,9888489	
Height from side 1 to side 2.					
18,2	*a* to *a* 211	192,8	5,0188095	4,9891513	4,98912
19	*b* to *b* 210	191	5,0186340	4,9889758	
19	*c* to *c* 214	195	5,0190242	4,9893660	
18	*d* to *d* 209	191	5,0186340	4,9889768	

Taking the mean of the above three means for the true length of the side of the cube, we have its content 124,1969 inches.

Measurement of the cylinder.

Each of the ends of the cylinder was crossed by Sir G. SHUCKBURGH by two diameters, the extremities of which were connected by lines drawn parallel to its axis. These lines, as well as the letters indicating their terminations, were distinctly visible.

Length of the cylinder.

The brass pieces being properly arranged, the excess of the distance between the dots and 6 inches, taken upon the scale between 52,1 and 58,1 was measured, which being added to 6 inches, and ,0296582 deducted, we obtain the length of the cylinder.

Cylinder. (Length.)

Reading of the Micrometer at the: Scale.	Dots.	Difference.	Distance between the Dots in Inches.	Length of the Cylinder in Inches.	Mean.
	a to *a*				
3,2	264,5	261,3	6,0254925	5,9958343	
	b to *b*				
3	270,5	267,5	6,0260973	5,9964391	5,99619
	c to *c*				
3	270	267	6,0260485	5,9963903	
	d to *d*				
3	267	264	6,0257558	5,9960976	
	a to *a*				
5,5	263,5	258	6,0251705	5,9955123	
	b to *b*				
3,2	270	266,8	6,0260290	5,9963708	
	c to *c*				
6,5	268	261,5	6,0255120	5,9958538	5,99590
	d to *d*				
3,5	265	261,5	6,0255120	5,9958538	
	a to *a*				
1	260	259	6,0252681	5,9956099	
	b to *b*				
1	266	265	6,0258534	5,9961952	
	c to *c*				
0	264	264	6,0257558	5,9960976	5,99590
	d to *d*				
1	261	260	6,0253656	5,9957074	

The mean of these three means being taken, we have 5,99600 for the length of the cylinder.

Diameter of the cylinder.

The brass pieces were most carefully placed, so that the dots were precisely in the direction of the diameters, and the surfaces tangents to the circumference. The distance between the dots was then compared with 4 inches on the scale, from 54 to 58, and the diameter obtained in the manner before described.

Cylinder. (Diameter.)

End 1.

Readings of the Micrometer at the Scale.	Readings of the Micrometer at the Dots.	Difference.	Distance between the Dots in Inches.	Diameter of the Cylinder in Inches.	Mean.
40	*a* to *b* 313,7	273,7	4,0267022	3,9972050	3,99721
41	*c* to *d* 318	277	4,0270242	3,9973660	
End 2.					
41	*a* to *b* 313	272	4,0265363	3,9968781	3,99703
41	*c* to *d* 316,2	275,2	4,0268485	3,9971903	

The mean of these means gives 3,99712 for the diameter of the cylinder.

On a subsequent day I repeated the measurement of the diameter, with the following results :

Cylinder. (Diameter.)

End 1.

Reading of the Micrometer at the		Difference.	Distance between the Dots in Inches.	Diameter of the Cylinder in Inches.	Mean.
Scale.	Dots.				
42,6	*a* to *b* 316,5	273,9	4,02672168	3,9970635	3,99702
43,4	*c* to *d* 316,5	273,1	4,02664364	3,9969854	
End 2.					
43	*a* to *b* 317,7	274,7	4,02679973	3,9971415	3,99725
42	*c* to *d* 319	277	4,02702412	3,9973669	

The mean of these measurements scarcely differs from that of the former. We have then the diameter of the cylinder,

by the 1st measurement - 3,99712
by the 2nd, - - - 3,99714

Mean - 3,99713

The length being 5,99600, and the diameter 3,99713, the capacity of the cylinder will be 75,2398 inches.

Measurement of the sphere.

On referring to Sir GEORGE SHUCKBURGH's account, it will be seen, that for the measurement of the sphere, a brass square was employed, the side of which was a very little longer than the diameter of the sphere. The sphere being placed within the square, and properly supported, a micro-

meter screw, which passed through one of the sides of the square, was brought in contact with the diameter of the sphere, and the reading of the micrometer head noted. The sphere being then removed, a brass rule of known length was put in its place, and the micrometer screw being brought in contact with the end of the rule, the difference between its length and the diameter of the sphere was obtained, from which the latter could, of course, be readily determined.

Sir GEORGE SHUCKBURGH had drawn three great circles in pencil upon the sphere, which, as well as the letters designating their intersections, remained perfect.

Having arranged the apparatus, the following measurements of the diameter of the sphere were taken, two of which may be termed equatorial, and the third polar, every precaution being used to prevent errors arising from difference of temperature.

Diameter of the Sphere.	Readings of the Micrometer.	
A to B	37	
C to D	38,5	38,5 mean.
E to F	40	

The rule being now placed in the square, the following were the readings of the micrometer on different trials.

51,5	
51,5	
51	51,33 mean.
51,3	

Hence, the diameter of the sphere exceeds the rule 12,83 divisions.

Second trial.

Diameter of the Sphere.	Readings of the Micrometer.	
A to B	38	
C to D	39,2	39 mean.
E to F	39,8	

The reading of the micrometer when the rule was placed in the square was 51,3; the diameter of the sphere, therefore, exceeds the rule 12,3 divisions.

Third trial.

Diameter of the Sphere.	Readings of the Micrometer.	
A to B	38	
C to D	38,3	38,43 mean.
E to F	39	

The readings of the micrometer when the rule was placed in the square were

51,7	
51,3	51,25 mean.
51,5	
50,5	

By this last trial, the diameter of the sphere exceeds the rule 12,82 divisions.

	Divisions.
By the 1st trial the diameter of the sphere exceeded the length of the rule - - -	12,83
By the 2nd - - - -	12,30
By the 3rd - - - -	12,82
Mean	12,65

which converted into inches, gives 0,0012281 for the excess of the diameter of the sphere above the length of the rule.

Length of the brass rule.

The brass rule was laid upon the standard scale, where it remained for two days before the measurement was made, in order that it might acquire the same temperature. The rectangular pieces of brass were then applied to its extremities, and the distance between the dots compared with the distance from 26 to 32 inches upon the scale, in the manner which has been before described.

Readings of the Micrometer at the		Difference, deducting 304 divisions. *
Scale.	Dots	
25	396	67
24	393,5	65,5
23	391,7	64,7
21	391	66
20,5	389,5	65
21	388,4	63,4
	Mean	65,3 =,0063609 of an inch.

* The distance between the Dots. See page 318.

The length of the brass rule from this appears to be 6,0063609 inches, which added to ,0012281, gives 6,00759 inches for the diameter of the sphere; whence we have its solid content 113.5264 inches.

It may now be useful to collect under one view the data furnished by Sir GEORGE SHUCKBURGH's experiments, and by the preceding measurements.

	Contents in Inches at 62°	Weight in Air, Grains.	Temp. of the Air.	Barom. Inches.	Weight of an equal bulk of Water, Grains.	Temperature when weighed in Water.
Cube - -	124,1969	32084,82	62°	29,00	31381,79	60°,2
Cylinder -	75,2398	21560,05	62	29,00	19006,83	60,5
Sphere - -	113,5264	28722,64	67	29,74	28673,51	66,0

From these data the weight of a cubic inch of distilled water in a vacuum at 62°, deduced from the cube, appears to be - 252,907 of Sir G. SHUCKBURGH's grains.
From the cylinder 252,851
And from the sphere 252,907

The mean of which is 252,888
which is equal to 252,722 grains of the Parliamentary Standard.

It is not my intention to enter into a detail of the various corrections necessary in the computation of the preceding results, as they may be found in the Appendix to the Third Report of the Commissioners of Weights and Measures.

London,
March, 1821.

XXIV. *An account of observations made with the eight feet astronomical circle, at the Observatory of Trinity College, Dublin, since the beginning of the year* 1818, *for investigating the effects of parallax and aberration on the places of certain fixed stars; also the comparison of these with former observations for determining the effects of lunar nutation. By the Reverend* John Brinkley, *D. D. F. R. S. and M. R. I. A. Andrews Professor of Astronomy in the University of Dublin.*

Read June 21, 1821.

The results of the observations which I now beg leave to lay before the Royal Society, were instituted with a view of discovering, if possible, the source of the differences that have existed between the results of former observations made here, and of others made at the Royal Observatory at Greenwich; and they will, it is imagined, be found to be useful relative to some other important points in astronomy.

My former observations of certain stars pointed out a deviation of about one second from the mean place, after having made all the usual corrections. Mr. Pond's observations pointed out no such deviations. The deviations that I had found agreed with the effects of parallax. The observations that I have since made, far more numerous than the former, concur in exhibiting the same results: in showing deviations in certain stars that can be explained by parallax. Every other suggested solution of the difficulty appears quite inadequate

thereto. It is, I think, nearly demonstrated, that no change of figure in the instrument has occasioned it, and that the uncertainties of the changes of refraction can have had only a very small share, if any, in producing the effect observed.

It is not the results of a mere repetition of observations that I now offer to the Royal Society, but the results of numerous sets of such observations as seemed best adapted to examine the question in all its bearings. Some of them seemed particularly adapted to disprove, if wrong, the explanation by parallax.

All attempts to arrive at results inconsistent with parallax have failed; so that, as far as the new observations are concerned, my former conclusions have been strengthened instead of weakened. I do not mean, however, to assert, that the subject is yet divested of the difficulties attendant on it from other sources. Some of the results that I have found, although in themselves in no manner inconsistent with parallax, will, justly perhaps with many, add to the difficulty of admitting the explanation by parallax. They will be unwilling to admit that many of the smaller stars are nearer to us than many of the brighter. That in a certain part of the heavens of considerable extent, many of the stars exhibit a sensible parallax. This however must be admitted, if my discordances result from parallax. If it be admitted, then several of the difficulties that have occurred by comparing my observations and those of Mr. POND, will be done away. But I shall defer a few remarks on this head, till I have given an account of my own observations, and of the results thereof.

The first set of results (Table 1) are from observations of

thirteen stars. These results contain the mean polar distance of each star reduced to January 1, 1819, the constant of aberration for each star, and the semi-parallax.

In deducing the quantity of parallax, the results must be affected by any uncertainty in the constant of aberration, since the times of the observations must necessarily be extended, so that the effects of aberration become sensible; and in like manner, in investigating the constant of aberration from observations of a given star, the parallax, if any, will be involved. Hence I adopted the following process in reducing the observations. The observed zenith distances of a given star were reduced to Jan. 1, 1819, by the common equations, taking the constant of aberration $= 20'',25$. The mean of these were taken. The correct mean zenith distance was supposed equal to this mean $-e$, the constant of aberration $= 20,25 + x$, and the semi-parallax $= p$. The equations of condition resulting from the respective observations thus contained three unknown quantities. These equations were reduced to three, by the method of making the sum of the squares of the errors a minimum. The solutions of these three equations give the values of e, x and p, and thence the values of the mean polar distance, constant of aberration, and semi-parallax, as stated in Table 1.

In regard to the selection of these stars, some were selected with a reference to my former results as to parallax; others, as being convenient for the investigation of the constant of aberration.

The parallax resulting for α Lyræ does not materially differ from my first determination. That of α Aquilæ is less than

before. Had $20\frac{1}{4}''$ been used for the constant of aberration, the result would have been only less by half a second than before.

In fact, the quantity of discordance does not differ from what I had before observed, but part of it now appears to arise from the constant of aberration being greater; a conclusion that will be deemed very important, should it be confirmed by future observations or other instruments. The parallax of Arcturus is somewhat less than before, and that of α Cygni considerably less. γ Draconis, as before, exhibits no parallax; the small negative result of $\frac{8}{100}$ of a second may safely be referred to the unavoidable errors of observation. The new results agree with the former, in showing that the Pole Star has no sensible parallax.

With respect to the constant of aberration, it is almost unnecessary to remark its important bearing on the theory of light. Should a decided difference in the quantity of that constant, for two stars, be established, it would be decisive against the undulatory system; and it would also show, that the corpuscular theory could not, without the addition of principles at present unknown, explain the phenomena of light. I trust the results here obtained will be found to possess some interest, and may induce others to pursue the same object. I dare not venture to draw any conclusion from them relative to these important points. The two stars η Ursæ Majoris and γ Draconis appear to point out a difference. These stars, by their proximity to the zenith and other circumstances, are well adapted for obtaining exact results. The observations of each star seem to be very

good, as will appear by Tables 4 and 5. A continuation of observations will, I hope, enable me to speak with confidence as to the identity or diversity of these numbers.

The constant for α Aquilæ will not be considered of so much weight as those of the higher stars, both on account of the more uncertain effects of refraction, and because only half the effect of aberration is visible in declination; although the influence of these circumstances is somewhat lessened by the greater number of observations.

The investigation of the constant of aberration by direct observations of zenith distance has not, that I am aware of, been attempted since those of BRADLEY, by the zenith sector. A century has nearly elapsed since his excellent observations were made. The results of M. DELAMBRE's investigations, relative to the velocity of light, as deduced from the eclipses of Jupiter's satellites, appeared to confirm in so strong a manner the mean of BRADLEY's results, that astronomers seem to have considered the point quite settled; but if I mistake not, one cause for this was the paucity of instruments adequate to so delicate an enquiry.

In considering the results with a view to the question of parallax, whether those that appear to point out parallax have not an origin in some cause unconnected with parallax: the first remark that offers itself is, that all the results furnish a positive parallax, if we except those small quantities in three of the stars which are quite within the limits of the unavoidable errors of observation. Might it not be expected that some of the stars would have furnished negative, as great as the positive quantities furnished by others? A considerable negative parallax would have been decisive. Again,

might it not have been expected that stars, in which the effect of parallax in declination is only a small part of the whole, would have shown a great parallax of declination as well as others, if the appearance of parallax is to be attributed to some other cause? Aldebaran, β Tauri, α Orionis, Castor, Procyon, Pollux, &c. are so situate, that only a small part of the whole parallax could affect the declination, and therefore if these stars had exhibited a change of place of a second or two, it could not arise from parallax. The results of observations made with reference to this are given in Table 2. by which it will be seen that no sensible change of zenith distance takes place in these stars. This appears a very important circumstance. Also, these stars in summer passing the meridian in the day time, and in winter in the night, the absolute temperatures of the air differ much more than in the summer and winter passages of α Lyræ and of α Aquilæ; therefore naturally greater irregularities might be expected as to the former stars, than as to the latter. This also appears deserving of notice.

To examine this question in another way, I instituted a set of observations on stars in the same part of the heavens as those in which I had found the discordances that appeared to arise from parallax. γ Draconis I had already observed; and the circumstance of its not exhibiting the same changes of place, as I had found in α Lyræ, appeared to afford a confirmation of my explanation. But this and α Aquarii are the only stars out of seventeen that appear not to be affected by similar changes. Hence a new difficulty. It certainly is not likely that those stars, some of them only of the fourth magnitude, should be nearer to us than some of the stars of

the first and second magnitudes. The stars γ and β Aquilæ appear to have a parallax as great, or greater, than α Aquilæ. The results of these observations are given in Table 3.

It is to be remarked, that these results cannot be considered nearly so exact as those of Table 1, because the observations are not nearly so numerous, and because the coefficients of p are in general much smaller. This latter circumstance could not be avoided in some of them, on account of their being too faint to observe in strong day light. For some of these stars also, the number of observations is so few, that a continuance of them may alter considerably the results; but with respect to others, this is not the case.

The value of p has not been deduced for α Aquarii, because of the smallness of its coefficients; but as this star shows a much less discordance than the others, it would afford, as well as γ Draconis, an argument favorable for the explanation by parallax, were not its zenith distance so great, that some uncertainty with respect to refraction may take place.

The results, contained in the three first tables, have been deduced from so many observations, that it is impossible that the principal conclusions, although relative to such minute quantities, can be materially affected by the variable errors of observation. If error exist, it must be from some cause not to be controlled by mere observations. Two causes suggested themselves, which seemed to require particular consideration.

1. The instrument being in such different states as to temperature in summer and winter, may, by changing its

figure or otherwise, occasion the discordances of the zenith distances. If this were so, it must exist for all stars; and Table 2 shows satisfactorily it does not exist; for those observed when the difference of temperatures is greater than when α Lyræ, α Aquilæ, &c. were observed.

The same is deduced from the observations of the Pole Star. If the instrument give different results for the same angle, it must appear in the co-latitude determined by the Pole Star at different seasons. The co-latitude found by contemporaneous observations above and below the pole, is not affected by any uncertainty in the quantity of aberration, or in the parallax of the Pole Star; it therefore affords a good criterion of the permanency of the scale of measurement of the instrument, if I may so express myself. The quantities are as follow:

	No. of Observations.	Z. D. Pole Star.	Co-latitude.
		° ′ ″	° ′ ″
Autumn	72 76 S.P.	34 57 21,24 38 16 11,84	36 36 46,53
Winter	72 64 S.P.	34 57 21,51 38 16 11,89	36 36 46,70
Spring	64 71 S.P.	34 57 21,26 38 16 11,71	36 36 46,49
Summer	72 60 S.P.	34 57 21,87 38 16 12,13	36 36 47,00

2. There may be an effect produced from the relative

temperatures of the external and internal air. The refractions have been computed by the internal thermometer. Now, at the summer observations of α Aquilæ and α Lyræ, &c. which take place between sunset and midnight, the external thermometer is oftentimes several degrees lower than the internal; the average is between $4°$ and $5°$. At the winter observations, the external thermometer at the hours when these stars are observed, averages only about one or two degrees lower. Hence, if the refractions were computed by the external thermometer, the results as to α Aquilæ and other stars of considerable zenith distance, would be less in favour of parallax.

But several circumstances induce me to conclude, that the true result is to be deduced from the internal thermometer. In a multitude of instances, were the external thermometer used, great discordances would take place. A great number of observations of circumpolar stars, made with a view to determine the constant of refraction, have given me nearly the same mean refraction as that determined by M. Delambre from a great mass of observations of his own, and of M. Piazzi, and which was also confirmed by the direct experiments of M. M. Biot and Arago on the refractive force of air, whereas had I computed by the external thermometer, the constant of refraction would have been much less. Also I have found the mean zenith distance, computed by the internal thermometer, when it stood several degrees higher than the external, fully equal to that found when the external and internal thermometer stood at the same height. This has been particularly the case as to the Pole Star below the Pole. The circumstances of the results I have obtained by

this star seem to render it certain, that my instrument, and the mode of proceeding I have adopted, cannot lead to any material error. It is evident, that the constant of aberration determined by zenith distances of the Pole Star, when observed above the pole, should be the same as that determined from observations of the same star when below the pole. The same holds as to the parallax. A comparison of results will show the degree of accuracy that may be expected to be obtained. Now, by a reference to Table 1, it appears that the constants of aberration only differ by a very small fraction of a second, and the results for the parallax agree in showing it to be insensible for this star. The passages of the Pole Star being separated by twelve hours, the circumstances are in a manner reversed at the opposite seasons of the maxima of aberration and parallax.

The more this argument is considered, the greater weight it will, I think, be found to have. The object of our enquiry is to ascertain, whether the instrument measures exactly the interval between the two places of a star at the opposite seasons. We have two modes of doing it for the Pole Star under opposite circumstances, and we find the same result. It must however be admitted, that it is difficult to ascertain, with exactness, the consequences of the differences of external and internal temperatures. It is a matter of some importance, and I hope to be able to make farther observations for ascertaining, more exactly, its bearing on the present question. In the mean time I beg to state distinctly, that, after reviewing all the circumstances of my observations, I do not consider my conclusions materially affected on this account.

Mr. Pond mentions, that in winter he endeavoured to

equalize the internal and external temperatures. Here the difference of temperatures is greatest after sunset in summer and autumn, except in extreme cold in winter; and the equalization of the temperatures cannot be easily affected without too great an exposure of the instrument to the external air. Partial currents might derange it, and occasion more uncertainty than that arising from the difference of temperatures. The room in which my instrument is placed, containing also the transit instrument, is of considerable dimensions, being thirty-seven feet long, twenty-three feet broad, and twenty-one feet high. The instrument is several feet from the shutters, which may be supposed a favourable circumstance. The apertures for observation are three feet wide.

Having thus given a detailed account of observations that have been principally instituted with a view of obtaining an explanation of the source of the difference of the results of my former observations and of those of Mr. POND, relative to parallax; it is with concern I state, that it contains not a trace of *any such explanation*. I have been unable to obtain *any result* that is *opposed* to my former conclusions.

It would be extremely important to ascertain the certainty of the results of an instrument, which, by its construction and principle of reversion, seems much better adapted to the present wants of astronomy than a mural circle. The advantage of referring each star to the apparent zenith point, and thus obtaining a knowledge of its motions without a reference to those of other stars, is easily appreciated. The advantage is also very great, of being able to observe a few minutes before the object arrives at the meridian, and, reversing the instrument, of then observing again. The zenith distance is thus

obtained completely, without a reference to the correction for collimation; and we are not obliged to depend for some days, perhaps, on the stability of the correction for collimation. We also are more likely, in this way, to improve our theory of refraction, because thus the irregular refractions of different days will not be mixed together. These considerations, independently of the interest of the question of parallax and aberration, lead me to dwell more on the discordance of the Greenwich observations, and of those made here, than otherwise I should be willing to do: and I am induced to offer a few brief remarks relative to the circumstances of the observations that have been adduced by Mr. POND, to prove the non-existence of a visible parallax.

1. The observations of the Greenwich mural circle are so implicated with each other, and the polar distances, even of the high stars, depend so much on the index error obtained by observations of those stars in which the uncertainties of refraction and of other data produce their effects, that it is not very extraordinary that the small quantities which I ascribe to parallax should not *distinctly* appear from the observations of the mural circle. There is indeed one exception to this explanation, which, I freely confess, occasions in my mind more difficulty than any other. This is in regard to γ Draconis and α Lyræ. According to the observations of Mr. POND, there is no difference between the relative places of these stars in summer and winter; and it is from a relative change of place I find in these two stars, that I adduce, what appears one of my strongest arguments for the parallax of α Lyræ. In this instance, the two instruments are completely at variance, and one of them must give an erroneous result.

2. The fixed telescope, used by Mr. Pond for the comparison of α Cygni and β Aurigæ, shows no relative changes of place that can be explained by attributing a parallax to α Cygni. This star formerly appeared to have a less parallax than others I had observed. My new observations give a much smaller quantity for it; but I am inclined to think the true quantity lies between my present and former results. Now admitting it to be half a second, no contradiction to this can be drawn from the observations by the fixed telescope, when those observations are carefully examined with a reference to the visible effects of the change of temperature

The fixed telescope used for α Aquilæ made the comparison by 55 *l* Pegasi. Now, the same maxima of parallax in declination of this star and of α Aquilæ occur within a few days of each other, so that it is completely the difference of parallax that is ascertained by comparing this star and α Aquilæ; and my results in Table 3 show, that in this part of the heavens we cannot conclude any thing as to the absolute parallax of one star by its relative parallax to that of another. Thus I cannot but venture an opinion, that nothing certain has hitherto been determined by the use of the fixed telescopes.

3. The results of the investigation of the parallax of α Aquilæ, by observations in right ascension, are still less satisfactory. The stars Mr. Pond has principally used for determining the error of the clock, are those in which I find the principal discordances, as will appear by a reference to the Greenwich observations; and consequently, those results ought to afford no appearance of parallax.

If stars opposite in right ascension be used, the utmost

exactness as to the stability and construction of the transit instrument and uniformity of the rate of the clock, is required. The Greenwich transit may be considered fully adequate; but it is evident the clock is not so perfect as it ought to be. In order to avail ourselves of this method, by stars opposite in Æ, at first view so plausible, of examining the question of parallax, skies much less changeable than those we are accustomed to will be required. As to this observatory, it rarely happens that a cloudless sky continues for twenty-four hours together. The entire of the observations from which my conclusions have been deduced will, I hope, soon be published. The particular results, therefore, of part only, are here added, that the nature of the observations, and the accuracy to be expected from them, may be more fully understood.

In Tables 4 and 5 will be seen the errors of each observation of η Ursæ Majoris and of γ Draconis, assuming as exact the results of all the observations of each star as to the mean zenith distance, parallax, and constant of aberration. These two stars have been chosen as examples, because in these the constants of aberration differ more than in other high stars. These stars being so near the zenith were observed on the meridian.

Of the 99 observations of η Ursæ Majoris.

2	In 2 observations the error exceeds 2″
18	In 20 observations the error exceeds 1
79	In 79 observations the error is under 1
99	

Of the 152 observations of γ Draconis.

5	In 5 observations the error exceeds 2″
48	In 53 observations the error exceeds 1
99	In 99 observations the error is below 1
152	

The errors of each observation of α Lyræ and of α Aquilæ are also given in Tables 6 and 7, adopting the results from all the observations of mean zenith distance, parallax, and constant of aberration, as exact. These stars have been selected on account of the great parallax deduced.

Of the 157 observations of α Lyræ.

2	In 2 the error exceeds 3″
6	In 8 the error exceeds 2
44	In 52 the error exceeds 1
105	In 105 the error is below 1
157	

Of the 135 observations of α Aquilæ.

2	In 2 the error exceeds 3″
8	In 10 the error exceeds 2
41	In 51 the error exceeds 1
84	In 84 the error is under 1
135	

In the above, the results of the observations of one day are considered as a single observation; but in computing the *values of* e, x, and p, each result was considered as having a weight proportional to the number of observations; that is,

each bisection of the star and reading off was considered as a distinct observation. The great improvement in the uniformity of the results is very apparent when two or four observations are made on the same day, by observing before and after the object has been on the meridian.

The greater errors occur according as the star is more remote from the zenith. This is doubtless occasioned by the irregularity of refraction, which is so very apparent when the object is within 10° or 15° of the horizon. It may be traced by my observations to within a few degrees of the zenith. On this account, when the object is 40° or 50° from the zenith, and great exactness is required, it will be necessary to increase the number of days of observation, rather than the number in the same day, that the irregularity may disappear from the mean.

I know not of any observations where the irregularity of refraction appears so distinctly as in mine. To illustrate this more fully, I have, in Table 8, added the observations of α Aquarii. This star I observed with a view of ascertaining whether it was subject to changes of place similar to what appeared in α Aquilæ. The mean results give a much less change of place, but the discordances which appear to belong to refraction are more fully apparent.

By observing before the star came to the meridian, and then reversing the instrument, using only the bottom microscope, I was enabled to get several results on the same day. In all the other stars three microscopes were used.

An inspection of Table 8 appears to show clearly the effects of this irregular refraction. Thus it is evident, that the differences between the results of the observations of

December 16, 1818, and December 28, 1820, must have been occasioned by the irregularities of refraction, as the respective observations of each day, in both positions of the face of the circle, are very consistent with one another. The same remark may be made as to the observations of August 17, 1819, and of September 6, 1820, &c. &c.

An illustration of the method of observing, &c. is given from γ and β Aquilæ in Tables 9 and 10.

The earlier observations of these stars were made on the meridian, and then the mean of the three microscopes, the refraction, and the mean zenith distance, January 1, 1819, as deduced from each observation, are given. Afterwards, when the observations were made off the meridian, the sidereal time elapsed between the observation and the passage over the meridian, is also given. The coefficients of x and p for each observation are also given.

In regard to the reductions of the observed zenith distance to the mean zenith distance: the precessions in N. P. D. corrected for proper motion, as given in the Nautical Almanac, were used. These annual variations agree so nearly with the annual variations deduced by using Mr. Bessel's precessions, and the proper motions deduced by a comparison of Mr. Bessel's results from Dr. Bradley's observations with the modern observations, that no inexactness can arise on this account.

The equation, in polar distance, used for lunar nutation was $-8'', 28 \text{ Sin } (Æ - \Omega) - 1'', 22 \text{ Sin } (Æ + \Omega)$

By a comparison of my observations of certain stars made 1809, 1814, and those made lately, I find this equation of lunar nutation $-8'', 06 \text{ Sin } (Æ - \Omega) - 1'', 19 \text{ Sin } (Æ + \Omega)$

If this should turn out, as I believe it will, more exact than the former, it will occasion no difference of results as to parallax and the constant of aberration.

The solar nutation I used was $-0'', 48$ Sin (Æ $-$ 2 ☉), not regarding the smaller term. With my lunar nutation, the solar nutation will be $= -0'', 52$ Sin (Æ $-$ 2 ☉) $-$ 0,02 Sin (Æ $+$ 2 ☉). That which I used, therefore, is sufficiently exact.

The small terms depending on 2 long. moon, have not been noticed on account both of their smallness and of the quickness of their period. The principal term of the nutation in North Polar distance depending on 2 long. of moon $= -0'', 08$ Sin (Æ $-$ 2 ☽),* which going through its period in the short space of a fortnight, can occasion no error in the results that I have obtained.

To the stars above given, for which the constants of aberration have been investigated, may be added α Cassiopeæ, α and β Cephei. The observations relative to parallax for these stars have not been sufficiently numerous to use the method of least squares.

* This term was stated in my paper in the Philosophical Transactions, 1818, as being $= -0{,}04$ Sin (Æ $-$ 2 ☽). I had adopted the numbers in the Mec. Cel. Tom. 2, p. 350, for the coefficients of Sin $2\nu'$ and cos $2\nu'$, but on examination I found that M. LAPLACE had omitted to multiply by $\lambda(3)$.

I may also remark, that in my two former papers on this subject, I unnecessarily, and in the first erroneously, introduced the effect of the elliptical motion of the earth in computing the aberration. The aberration in N. P. D. computed by the formula m cos (☉ $\sim$ K) differs from the true quantity by the constant quantity $\frac{1}{60} m$ cos ($9^s. 9^o. \sim$ K) and therefore the mean place of the star needs only to be regarded.

	No. of Ob.	N. P. D. Jan. 1, 1819.	Constant of aberration.
		° ′ ″	″
α Cassiopeæ	87	34 27 23,47	20,30—,14p
α Cephei	100	28 10 42,74	20,56—,39p
β Cephei	62	20 13 57,05	20,20—,38p

In my paper, Philosophical Transactions 1819, the constant for α Ursæ Majoris was given. In Table 1, β Ursæ Majoris has been introduced; both could not be observed at the same time; and having formerly intended to deduce the constant of aberration from the mean of a great number of stars, β Ursæ Majoris was observed.

The importance of the enquiry relative to the velocity of light, has since induced me to multiply as much as possible observations of the same star, and therefore the observations of α Ursæ Majoris have not been resumed.

Lunar Nutation.

A comparison of zenith distances of certain of the stars that I observed in the years 1809-1814, and of the zenith distances of the same stars observed in the years 1818-1820, has given the following results relative to lunar nutation.

It is almost unnecessary to remark, that those stars only were used in which the nutation at each period was nearly a maximum, with contrary signs. Two circumstances are particularly required to obtain the most accurate results.

1. That a comparison of results should be deduced from observations made by the *same* instrument.

2. That observations should be continued through a whole period of the lunar nodes, in order to ascertain, with exactness, the annual variation of zenith distances for each star.

The latter condition can only be fulfilled hereafter for my instrument. In the mean time, no material uncertainty can arise from the want thereof. The accurate reductions of Dr. BRADLEY's observations by Mr. BESSEL, have given us, with much exactness, the mean N. polar distances in 1755 of the stars I have used. Three periods nearly of the lunar nodes intervened between 1755 and my former observations. Hence, assuming the change from precession, as deduced by Mr. BESSEL, the proper motion of each star was obtained; this proper motion was then applied to the precession of each star for the years 1815 and 1816, which was also deduced by help of Mr. BESSEL's precessions. The annual variation of each star, thus obtained, for the middle of the interval between my two sets of observations, was used in connecting those sets to determine the exact effect of lunar nutation.

As the lunar nutation in N. P. D. used was,

$$-8'',28 \text{ Sin } (Æ - ☊) - 1'',22 \text{ Sin } (Æ + ☊)$$

and therefore the greatest term of the nutation of obliquity of ecliptic $= 9'',50 \cos ☊$; I supposed the true coefficient of this latter $= 9'',50\,(1 + y)$ and then found as follow:

	Number of Observations in 1808-1814.	Number of Observations in 1818-1820.	Equations deduced.	Greatest coeff. of Nutation of Ob. Eclip.
Capella	30	96	$54'',20 + 8'',49y = 53'',50 - 7'',79y$	$9'',09$
β Tauri	18	84	$21,73 + 8,66y = 21,65 - 7,63y$	9,45
α Orionis	18	148	$9,24 + 8,81y = 7,98 - 7,09y$	8,75
Castor	10	66	$30,23 + 8,92y = 30,42 - 5,18y$	9,62
Procyon	16	136	$8,41 + 8,91y = 7,30 - 4,88y$	8,74
Pollux	10	65	$44,98 + 8,79y = 44,29 - 4,81y$	9,01
γ Draconis	27	132	$7,54 - 8,65y = 7,90 + 5,80y$	9,26
α Lyræ	126	155	$42,69 - 9,14y = 42,94 + 7,89y$	9,36
α Aquilæ	76	238	$4,94 - 8,74y = 5,10 + 7,42y$	9,40
α Cygni	47	120	$42,15 - 7,48y = 42,77 + 4,97y$	9,03
	378	1240		9,25

On account of the small number of observations of some of the stars at the first period, it appears better to take the mean, by giving each result a weight proportional to the number of observations of each star at the first period. The mean result so obtained is $9'',25$. With this result (omitting the small terms depending on 2 long. ☊)

The nutation in N. P. D. $= - 8'',06 \sin(\text{Æ} - ☊) - 1'',19 \sin(\text{Æ} + ☊)$

The nutation in Æ $= (-8,06 \cos(\text{Æ} - ☊) - 1'',19 \cos(\text{Æ} + ☊)) \cot.$ N. P. D.

Equation of equinoxes in Æ $= - 15'',86 \sin ☊$

Equation of equinoxes in long. $= - 17'',29 \sin ☊$

Equation of obliquity ecliptic $= 9'',25 \cos ☊$

With the above nutation, the mass of the moon $= \frac{1}{80}$, that of the earth being unity; and the force of the moon on the sea $= 2\frac{1}{5}$, that of the sun being unity.

Had the former observations for each star been as numerous as those in the latter, it cannot I think be doubted, that the discordances of the results would have been less. The discordance between the greatest and least result is less than one second. Hence it might perhaps be inferred, that,

supposing the constant of aberration for each star the same, two results in Table 1 should not differ nearly so much as by 1″, on account of the great number of observations used in deducing the results of that Table.

The discordances between my observations and those made at Greenwich may, by some, be considered as showing the great precision of modern observations, when it is understood that the whole extent of the absolute difference between the results of the observations of the Astronomer Royal, and of those made here, is only about one second. But, independently of the interest of the question of parallax, it is highly important to ascertain the origin of this small difference. It may instruct as to the limit of accuracy actually to be attained to, when apparently there should exist no limit.

It will also appear, should any of the results that I have found be inexact, that the delicacy of an instrument cannot be appreciated by giving correctly some of the smaller motions, real or apparent, that occur, because the same instrument may, as to others, entirely mislead. Whatever may be the ultimate determinations, it is hoped, that the long and tedious exertions that have been used in obtaining these results, will not be found to have been entirely without use.

TABLE I.

	No. of Days of Observation.	No. of Observations, 1818-1821.	N. P. D. Jan. 1, 1819, co. lat. 36° 36′ 46″,5.	Const. of Aberration.	Semi-parallax or p.
			° ′ ″	″	″
Polaris - -	77	343	1 39 24,95	20,18	— 0,03
Polaris S. P.	80	337	1 39 25,16	20,12	+ 0,12
β Ursæ Majoris	75	75	32 38 59,61	20,16	+ 0,02
γ Ursæ Majoris	105	105	35 17 55,15	20,48	+ 0,39
ε - -	109	109	33 3 19,54	20,29	+ 0,33
ζ - -	94	94	34 7 34,65	20,23	+ 0,28
η Ursæ Majoris	99	99	39 46 47,18	20,76	+ 0,13
Arcturus - -	94	259	69 52 13,66	20,04	+ 0,61
β Ursæ Minoris	53	131	15 6 17,74	20,49	— 0,13
α Ophiuchi -	97	228	77 17 58,23	20,39	+ 1,57
γ Draconis -	152	152	38 29 7,51	19,86	— 0,08
α Lyræ - -	157	227	51 22 42,84	20,36	+ 1,21
α Aquilæ -	135	320	81 36 5,11	21,32	+ 1,57
α Cygni - -	94	154	45 21 42,30	20,52	+ 0,33

The heads of the respective columns sufficiently explain the numbers of this Table. It may be mentioned, that the stars near the zenith have only been observed on the meridian, and therefore the number of days of observations of these stars are the same as the number of observations. The other stars have often been observed near the meridian on each side. The stars 30° or more from the zenith have been observed twice before the reversion of the instrument, and twice after; the Pole Star occasionally still oftener. The other stars on the south side of the zenith only once in each position of the circle.

The small negative values of p have been put down to show the precise results; these, it is evident, may be wholly attributable to the unavoidable errors of observation; and

therefore to the extent of these quantities at least, the other results cannot be depended on.

The whole number of observations is 2633, and the mean of all the constants of aberration is 20″,37.

TABLE II.

	No. of winter Observations.	Mean N. P. D. Jan. 1, 1819, by winter Observations.	N. P. D. by summer Observations.	No. of summer Observations.
		° ′ ″	″	
α Ariétis - -	40	67 23 53,43	53,08	36
Aldebaran -	65	73 51 49,35	49,09	30
β Tauri - -	52	61 33 21,76	21,66	38
α Orionis -	82	82 38 9,29	9,17	65
Castor - -	34	57 43 29,94	30,52	33
Procyon - -	64	84 19 8,44	8,41	73
Pollux -	36	61 32 45,00	44,96	29
	Spring.			Autumn.
Regulus - -	21	77 9 7,45	7,58	12
β Leonis - -	20	74 24 57,91	57,53	27

The mean of the differences of the 343 summer observations, and of the 414 winter observations, is only $\frac{6}{100}$ of a second.

TABLE III.

	No. of Observations in summer and winter.	Zenith Distance Jan. 1, 1819, const. of Aberration for each star = 20",25 + x and p = semi-parallax.	Values of p.
α Cor. Borealis	50 S 28 W	26° 3' { 23",21 + 0,29x + 0,62p 24,72 — 0,24x — 0,68p }	p = 1",16 — 0,41x
α Serpentis	45 S 36 W	46 23 { 2,64 + ,09x + ,44p 3,83 — ,23x — ,42p }	p = 1,35 — 0,37x
δ Aquilæ	37 S 27 W	50 37 { 26,61 + ,24x + ,36p 28,55 + ,36x — ,25p }	p = 3,20 + 0,20x
β Cygni	34 S 26 W	25 48 { 1,38 + ,30x + ,69p 2,31 + ,69x — ,31p }	p = 0,93 + 0,39x
γ Aquilæ	39 S 46 W	43 12 { 23,92 + ,30x + ,43p 25,67 + ,38x — ,37p }	p = 2,19 + 0,10x
β Aquilæ	36 S 38 W	47 25 { 25,85 + ,28x + ,37p 27,50 + ,35x — ,33p }	p = 2,36 + 0,10x
γ Cygni	33 S 26 W	13 42 { 15,41 + ,55x + ,64p 16,40 + ,58x — ,60p }	p = 0,80 + 0,02x
ε Delph.	28 S 17 W	42 41 { 29,84 + ,41x + ,33p 31,12 + ,36x — ,40p }	p = 1,75 — 0,07x
α - - -	11 S 9 W	38 6 { 23,48 + ,41x + ,42p 24,86 + ,42x — ,42p }	p = 1,64 + 0,01x
λ Cygni - -	14 S 10 W	17 33 { 21,53 + ,49x + ,63p 23,01 + ,65x — ,49p }	p = 1,32 + 0,14x
ν - -	34 S 18 W	12 54 { 42,62 + ,51x + ,67p 43,24 + ,73x — ,44p }	p = 0,56 + 0,20x
γ Equulei	12 S 9 W	43 58 { 40,35 + ,41x + ,29p 42,34 + ,34x — ,38p }	p = 2,97 — 0,10x
α - - -	10 S 10 W	48 52 { 51,82 + ,39x + ,19p 53,39 + ,26x — ,37p }	p = 2,80 — 0,23x
ε Pegasi	26 S 21 W	44 20 { 10,80 + ,42x + ,25p 11,99 + ,19x — ,45p }	p = 1,70 — 0,33x
κ Aquarii	36 S 30 W	54 34 { 52,55 + ,37x + ,11p 53,25 + ,01x — ,39p }	
55 *l* Pegasi	33 S 45 W	44 57 { 9,64 + ,41x + ,15p 9,94 + ,16x — ,39p }	

The above results will appear very extraordinary; and although they are explained by parallax, yet many circumstances of these stars, both as to magnitude and position, will much weaken that explanation, and, on the whole, the results may be thought to have encreased the difficulties of the subject. It is evident, that, for most of these stars, the terms depending on x can have little influence, considering the smallness of the coefficients, and the probable small values of x.

TABLE IV. η Ursæ Majoris.

Date of Observation.	Obs.	Face, East or West.	Error of Observation.	Date of Observation.	Obs.	Face, East or West.	Error of Observation.	Date of Observation.	Obs.	Face, East or West.	Error of Observation.
1818.			"	1819			"				"
June 17	1	W	—0,65	Oct. 27	1	W	+0,41	May 19	1	E	—0,14
20	1	E	+0,83	29	1	W	+0,66	22	1	E	+0,13
22	1	W	—0,07	Nov. 1	1	E	+0,53	24	1	W	—1,04
28	1	E	+0,42	2	1	W	+0,66	June 23	1	W	—0,24
29	1	W	+0,39	5	1	E	—0,62	24	1	E	+0,21
July 2	1	E	—0,56	7	1	W	—0,06	25	1	W	—1,55
5	1	W	+1,39	15	1	E	+0,19	26	1	E	—0,80
15	1	E	+0,39	20	1	W	—0,91	27	1	E	—0,85
Nov. 2	1	E	—0,09	21	1	E	+0,09	28	1	W	+0,33
6	1	W	+0,88	23	1	W	—0,07	July 6	1	W	+0,64
9	1	E	+1,51	25	1	E	—0,11	7	1	E	+0,41
14	1	W	+0,47	Dec. 2	1	W	+0,23	8	1	W	—0,67
22	1	E	—1,19	15	1	W	—0,79	10	1	E	—0,63
23	1	W	+0,45	24	1	E	+1,38	13	1	W	+0,03
28	1	E	—1,18	30	1	W	+0,51	15	1	E	+0,17
Dec. 1	1	E	+0,09	1820				Oct. 4	1	W	+0,36
2	1	W	—1,48	Jan. 10	1	E	—1,19	27	1	E	+0,04
4	1	E	+0,21	April 14	1	E	—0,94	28	1	W	+0,29
8	1	W	+2,47	18	1	W	—0,14	29	1	E	—0,14
15	1	E	+1,66	21	1	E	+2,76	31	1	W	+0,35
17	1	W	+0,87	22	1	W	+1,35	Nov. 1	1	E	—0,36
1819				24	1	E	+0,04	2	1	E	+0,36
June 24	1	W	—0,39	25	1	W	+0,18	16	1	W	—0,16
July 3	1	E	—0,43	30	1	E	+1,09	17	1	E	—0,92
4	1	W	+0,81	May 1	1	W	—0,14	21	1	W	—1,82
8	1	E	+0,88	3	1	E	—0,78	23	1	E	—0,72
14	1	W	+0,84	4	1	W	+0,01	Dec. 10	1	W	—0,74
20	1	E	—1,01	6	1	E	—0,75	18	1	W	—0,58
21	1	E	—0,66	8	1	W	—0,23	1821			
24	1	W	—0,33	10	1	W	—1,52	Jan. 2	1	E	—0,27
27	1	E	+0,87	12	1	E	+0,11	3	1	W	—1,07
29	1	W	—1,25	13	1	E	—0,68	16	1	W	+0,46
31	1	W	—0,30	17	1	W	+0,99	19	1	E	—0,45
Oct. 15	1	E	+0,31	18	1	W	+1,39	26	1	W	—0,44

TABLE V. γ Draconis.

Date of Observation.	Obs. Face, East or West.		Error of Observation.	Date of Observation.	Obs. Face, East or West.		Error of Observation.	Date of Observation.	Obs. Face, East or West.		Error of Observation.
1817.				1819.				1820.			
Nov. 11	1	W	—0,17	Jan. 31	1	W	—0,06	March 2	1	E	—0,64
16	1	E	+1,74	Feb. 1	1	E	—1,05	8	1	W	—0,64
17	1	W	—2,01	July 14	1	E	+0,99	June 28	1	E	+0,32
Dec. 6	1	W	+0,23	15	1	W	—0,22	July 5	1	W	+1,66
10	1	W	+0,76	20	1	E	+0,21	7	1	W	+0,66
21	1	W	+1,80	21	1	W	+0,31	8	1	E	+0,30
22	1	E	—0,22	24	1	E	—1,07	10	1	W	+0,78
23	1	E	—0,41	27	1	W	—1,02	13	1	E	+0,69
24	1	W	+0,37	28	1	W	—1,61	15	1	W	—0,46
26	1	E	—0,88	29	1	E	+0,46	18	1	E	+0,60
27	1	E	+0,20	30	1	W	+0,33	19	1	W	—0,83
30	1	W	—0,06	31	1	E	+1,28	21	1	E	—1,41
1818.				Aug. 2	1	W	+0,85	24	1	W	—0,51
Jan. 5	1	E	—1,45	4	1	E	—1,26	28	1	E	0,00
7	1	W	—0,75	7	1	W	+0,86	Aug. 1	1	W	—1,41
20	1	E	—0,17	8	1	E	—1,71	31	1	E	+0,68
23	1	W	—0,88	9	1	E	+0,36	Sept. 1	1	W	+0,49
30	1	E	—0,38	12	1	W	—1,27	2	1	W	+2,43
Feb. 1	1	E	—1,48	15	1	E	+0,95	6	1	E	+1,09
July 19	1	W	+0,38	18	1	W	—1,31	8	1	E	—0,08
25	1	E	+0,35	19	1	E	—1,11	15	1	W	+0,18
27	1	W	+0,70	20	1	W	+1,12	16	1	W	—0,06
Aug. 1	1	E	+1,97	21	1	E	—0,10	17	1	E	+1,24
2	1	W	—0,37	22	1	W	+0,81	18	1	E	+0,47
6	1	E	—1,61	Sept. 10	1	W	+2,45	20	1	W	+0,34
7	1	W	+0,80	14	1	E	—0,96	Oct. 4	1	E	—1,25
10	1	E	—0,93	16	1	W	+0,21	17	1	E	+0,73
11	1	W	+1,61	20	1	E	+0,17	25	1	W	+0,21
12	1	E	—0,55	22	1	E	—0,14	Nov. 2	1	W	—0,27
13	1	W	—0,34	23	1	W	+0,70	4	1	E	—0,76
14	1	E	—0,75	27	1	E	—1,22	10	1	W	—1,03
15	1	W	—2,94	Oct. 2	1	W	—1,72	Dec. 19	1	W	—0,77
16	1	E	+1,20	3	1	E	+0,16	27	1	E	—0,71
Nov. 24	1	W	+0,49	4	1	W	—0,16	1821.			
Dec. 5	1	E	—1,26	20	1	W	—0,34	Jan. 2	1	E	+1,38
7	1	W	—0,44	24	1	E	+0,98	13	1	W	—1,46
9	1	E	—0,99	26	1	W	—0,47	19	1	W	—0,61
15	1	W	—0,12	29	1	E	+0,66	Feb. 1	1	E	—0,72
28	1	W	—1,72	30	1	W	—0,01	4	1	E	+0,36
29	1	E	+3,00	Nov. 8	1	E	+1,10	6	1	W	—0,48
1819.				Dec. 13	1	W	+0,27	7	1	W	—0,31
Jan. 8	1	W	+1,64	14	1	E	—0,15	8	1	E	—0,41
10	1	E	—0,79	15	1	W	—1,04	10	1	E	—1,75
11	1	W	+0,39	22	1	E	+1,62	14	1	W	+0,78
12	1	E	+1,22	24	1	E	+0,77	19	1	W	+0,50
17	1	W	+0.72	25	1	W	+1,43	24	1	E	—0,47
18	1	E	+0,42	27	1	E	+0,64	27	1	W	+0,18
19	1	W	—1,08	28	1	W	+0,50	March 9	1	W	+1,35
20	1	E	—1,14	1820.				11	1	E	—0,87
22	1	W	+0,44	Jan. 3	1	W	+1,75	12	1	W	—1,73
25	1	E	—0,31	15	1	E	+1,12	13	1	E	+1,71
29	1	W	—1,61	27	1	E	—0,18	15	1	W	+1,02

TABLE VI. α Lyræ.

Date of Observation.	Obs. Face, East or West.		Error of Observation.	Date of Observation.	Obs. Face, East or West		Error of Observation.	Date of Observation.	Obs. Face, East or West.		Error of Observation.
1818.			″	1819.			″	1820.			″
July 14	1	W	−0,77	Feb. 24	1	W	+0,21	Mar. 22	1 E	1 W	−0,41
15	1	E	−0,26	July 3	1	E	−2,80	24	1 E	1 W	−0,13
16	1	W	−0,18	4	1	W	−0,06	25	1 E	1 W	+0,84
17	1	W	−2,67	14	1	E	−1,12	April 5	1 W	1 E	−0,64
19	1	E	−1,65	15	1	W	+0,32	6	1 E	1 W	−0,73
24	1	E	+0,41	20	1	E	+0,16	July 5	1 W	1 E	+1,78
25	1	E	−2,35	21	1	W	+0,59	7	1 W	1 E	+0,30
27	1	W	−1,37	24	1	E	+0,20	8	1 E	1 W	+0,44
Aug. 1	1	E	+1,13	28	1	W	−1,63	10	1 W	1 E	+0,82
2	1	W	−0,21	29	1	E	−0,42	13	1 E	1 W	+0,52
6	1	E	−1,66	30	1	W	−1,32	15	1 W	1 E	−0,48
7	1	W	+1,50	Aug. 2	1	W	−0,71	18	1 E	1 W	+1,55
9	1	W	+0,70	4	1	E	−0,77	19	1 W	1 E	+1,76
10	1	E	−0,39	7	1	W	−1,28	24	1 W	1 E	+0,84
11	1	W	−0,28	9	1	E	+0,10	25	1 E	1 W	+0,77
12	1	E	+0,43	15	1	W	−3,08	Sept. 12	1 W	1 E	+0,96
13	1	W	−0,55	18	1	E	+0,28	15	1 W	1 E	+0,17
14	1	E	+0,07	19	1	W	+0,01	18	1 E	1 W	+0,48
15	1	W	−0,64	21	1	E	−0,72	20	1 W	1 E	−0,06
16	1	E	−0,34	23	1	W	−0,60	Oct. 4	1 E	1 W	+0,47
Oct. 16	1	W	+0,96	27	1	E	−2,69	5	1 E	1 W	+1,15
17	1	E	+1,98	31	1	W	+1,38	18	1 W	1 E	+0,82
19	1	W	−0,72	Sept. 8	1	W	+1,55	25	1 W	1 E	+0,27
20	1	E	−0,91	10	1	W	+1,34	Nov. 1	1 W	1 E	−0,17
26	1	E	+1,12	12	1	E	−0,93	2	1 W	1 E	−0,81
Nov. 2	1	W	+0,23	14	1	E	−2,85	Dec. 2	1 W	1 E	+0,39
3	1	E	−1,22	16	1	W	−0,32	11	1 W	—	+0,07
7	1	E	−0,62	20	1 E	1 W	+0,82	19	1 W	1 E	+0,44
8	1	W	−0,19	21	1	E	−0,99	23	E	1 W	−0,86
24	1	W	+1,16	22	1	W	−1,05	28	1 E	1 W	+0,30
Dec. 5	1	E	−0,97	23	1	W	−0,91	1821.			
7	1	W	+0,75	Oct. 2	1 E	1 W	−0,46	Jan. 2	1 E	1 W	+0,45
9	1	E	+0,01	4	1 E	1 W	+0,63	13	1 W	1 E	+0,33
15	1	W	−2,44	17	1	E	+1,73	19	1 W	1 E	−0,20
16	1	E	+0,32	20	1 E	1 W	+0,25	Feb. 1	1 E	1 W	−1,45
21	1	W	−3,41	29	1	E	+1,68	2	1 W	1 E	−0,85
22	1	W	−1,38	30	1	W	+0,20	6	1 W	1 E	−0,58
1819.				Nov. 2	1	E	+1,20	8	1 E	1 W	+0,31
Jan. 8	1	W	+1,22	8	1	W	−0,87	10	1 E	1 W	−0,06
10	1	E	−1,58	Dec. 13	1 W	1 E	−0,61	14	1 W	1 E	+1,42
11	1	W	−1,40	15	1	W	−1,95	19	1 W	1 E	−1,36
12	1	E	−0,88	16	1 E	2 W	−1,19	24	1 E	1 W	+1,10
17	1	W	−0,05	23	1	E	+1,06	27	1 W	1 E	−0,62
18	1	E	−1,30	26	2 W	2 E	+0,92	Mar. 9	1 W	1 E	+0,25
19	1	W	+0,53	29	1 W	1 E	+1,05	10	1 W	1 E	−0,03
20	1	E	−0,91	30	1 W	1 E	+1,25	11	1 E	1 W	−0,23
25	1	E	−1,20	1820.				12	1 W	1 E	−0,54
29	1	W	+0,18	Jan. 2	1 E	1 W	+0,92	13	1 E	1 W	+1,08
31	1	W	−0,93	5	1 W	1 E	+0,36	15	1 W	E	+1,23
Feb. 1	1	E	−1,58	15	1 E	1 W	+0,88	19	1 W	E	+1,26
5	1	W	−0,40	16	1 W	1 E	+0,50	21	1 E	1 W	+1,56
6	1	W	−0,08	Mar. 2	1 E	1 W	+0,28	22	1 W	1 E	+0,44
9	1	E	−0,16	8	1 W	1 E	+0,13				
21	1	E	−0,90	19	1 E	1 W	−0,81				

TABLE VII. α Aquilæ.

Date of Observation.	Obs. Face, East or West.		Error of Observation.
1818.			
July 14	1	W	—0,50
17	1	E	—1,33
24	1	W	—1,85
25	1	E	—1,89
Aug. 1	1	E	—0,72
2	1	W	—2,12
6	1	E	—3,88
9	1	W	—1,65
10	1	E	—1,66
11	1	W	—1,86
12	1	E	—1,60
13	1	W	—1,95
14	1	E	—0,57
15	1	W	—0,42
16	1	E	—0,06
Oct. 16	1	W	—0,62
17	1	E	—0,52
21	1	W	—0,05
Nov. 1	1	E	—0,07
2	1	W	—1,22
3	1	E	—1,90
5	1	W	—0,44
7	1	E	+0,30
8	1	W	+0,65
14	1	E	+0,22
20	1	W	+0,30
23	1	E	—0,11
24	1	W	—0,51
Dec. 7	1	W	+1,46
8	1	E	—2,24
9	1	W	+1,85
15	1	E	—0,68
16	1	W	—0,26
18	1	E	+1,03
21	1	W	—1,75
22	1	E	—0,01
29	1	E	+1,68
30	1	W	—0,69
1819			
Jan. 2	1	E	+0,65
19	1	W	—0,98
20	1	E	—4,16
22	1	W	—0,54
Feb. 1	1	E	—1,08
6	1	W	—0,79
9	1	E	—0,92

Date of Observation.	Obs. Face, East or West.		Error of Observation.
1819.			
Feb. 12	1	W	—1,17
13	1	E	+0,37
15	1	W	—1,08
21	1	E	+0,02
22	1	W	+0,27
23	1	E	—0,73
24	1	W	—0,01
Mar. 3	1	E	+2,24
Aug. 4	1	E	—2,65
7	1	W	—1,47
9	1	E	—1,60
11	1	W	—2,48
12	1	E	+0,48
15	1	W	—1,65
18	1	E	+0,32
20	1	W	—1,14
21	1	E	—0,34
22	1	W	+0,14
24	1	E	+1,63
27	1	E	+1,78
Sept. 1	1	W	+2,21
3	1	E	—1,74
6	1	W	+2,96
7	1	E	+0,29
8	2 E	2 W	+0,12
10	2 E	2 W	—0,39
11	2 E	2 W	+0,49
12	2 E	2 W	+0,38
14	2 E	2 W	—0,45
16	2 E	2 W	—0,12
19	2 E	2 W	+0,36
20	2 E	2 W	+0,07
21	2 E	2 W	+1,89
Dec. 3	2 E	1 W	—1,03
8	2 E	2 W	+0,14
13	2 E	2 W	+0,21
14	2 E	2 W	—0,64
15	2 E	2 W	—0,59
23	2 E	2 W	—0,34
26	2 E	2 W	—0,08
29	2 E	2 W	—0,63
1820.			
Jan. 3	—	2 W	—0,48
6	2 E	2 W	—0,42
Mar. 8	2 E	2 W	+1,03
16	2 E	2 W	+1,63

Date of Observation.	Obs. Face, East or West.		Error of Observation.
1820.			
Mar. 19	2 E	2 W	—1,09
20	2 E	—	—0,31
22	—	2 W	—0,79
24	2 E	2 W	+0,33
28	2 E	2 W	+0,15
29	2 E	2 W	—0,44
April 5	2 E	—	—1,38
6	—	2 W	—2,04
14	2 E	2 W	+0,55
July 28	2 E	2 W	—0,12
31	2 E	2 W	+1,01
Aug. 1	2 E	2 W	+0,08
4	2 E	2 W	+0,50
5	2 E	2 W	+1,10
9	2 E	2 W	+0,47
10	2 E	2 W	+0,58
17	2 E	2 W	—0,94
18	2 E	2 W	+0,06
19	2 E	2 W	—0,05
Sept. 15	2 E	2 W	+0,16
17	2 E	2 W	—0,34
18	2 E	2 W	—0,97
20	2 E	2 W	+0,19
Oct. 4	2 E	2 W	+1,63
17	2 E	2 W	+0,36
18	2 E	2 W	+1,75
4	2 E	—	+1,18
25	2 E	2 W	+0,06
28	2 E	2 W	+0,83
Dec. 2	2 E	2 W	—0,27
23	2 E	2 W	—0,71
28	2 E	2 W	+0,47
1821.			
Jan. 19	2 E	2 W	—1,31
Feb. 1	2 E	2 W	—1,13
9	2 E	2 W	—0,27
10	2 E	2 W	—0,01
19	2 E	2 W	+0,33
24	2 E	2 W	+0,57
27	2 E	2 W	+1,13
Mar. 3	2 E	2 W	+1,38
9	2 E	2 W	+0,46
10	2 E	2 W	+0,12
11	2 E	2 W	+0,27
13	2 E	2 W	+0,86
15	2 E	2 W	+1,39

In the above, it might have been better to have omitted the observations of August 6, 1818, and January 20, 1819. No difference, however, would have taken place in the total results; and it may be proper here to remark, that no result has been omitted, except some error is clearly shown by the circumstances of the observation; and this has not happened in any thing like ten instances in above 4000 observations.

TABLE VIII. α Aquarii.

	Face of Circle.	Time from Meridian.	Bottom Microscope.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1818.		′ ″	° ′ ″	′ ″	° ′ ″	
Sept. 2	E	7 38,2	54 35 43,8	1 20,22	54 34 53,32	
	E	5 50,7	35 9,4	1 20,20	53,61	+,37 *x*
	E	3 48,2	34 41,0	1 20,18	53,49	
In.	W	4 46,8	33 29,6	1 20,18	52,82	+,12 *p*
Bar. 29,59	W	6 36,3	33 59,2	1 20,20	52,59	
Therm.	W	8 47,3	34 48,0	1 20,22	53,09	
Int. 53°½						
Ext. 49°½					53,15	
4	E	7 52,7	35 47,7	1 19,52	51,25	
	E	5 49,7	35 8,6	1 19,48	52,48	+,37 *x*
In.	W	3 12,8	33 12,6	1 19,47	53,22	+,11 *p*
Bar. 29,60	W	5 24,8	33 38,2	1 19,48	51,56	
Therm.	W	7 23,8	34 15,8	1 19,52	52,70	
Int. 58°						
Ext. 53°					52,24	
Dec. 16	W	8 30,8	34 34,9	1 23,53	50,65	
	W	6 51,8	33 58,4	1 23,51	50,55	+,02 *x*
	W	5 6,8	33 28,9	1 23,48	51,14	
In.	E	3 33,2	34 38,4	1 23,48	51,76	—,39 *p*
Bar. 29,87	E	5 28,2	35 3,7	1 23,51	52,24	
Therm.	E	7 6,2	35 31,5	1 23,53	50,56	
Int. 39°						
Ext. 36°					51,15	
18	E	11 38,9	37 34,7	1 23,36	51,24	
	E	7 49,9	35 47,9	1 23,26	51,12	+,01 *x*
	E	4 56,9	34 55,7	1 23,22	51,82	
In.	W	4 58,1	33 28,7	1 23,28	52,90	—,39 *p*
Bar. 29,80	W	8 57,1	34 48,0	1 23,36	52,64	
Therm.	W	12 9,1	36 26,2	1 23,43	53,9[illegible]	
Int. 39°						
Ext. 35°					52,27	
21	W	10 16,2	35 24,7	1 23,30	52,96	
	W	7 9,2	34 8,0	1 23,24	54,19	—,01 *x*
	W	4 41,2	33 24,1	1 23,18	52,18	
In.	E	5 47,8	35 10,2	1 23,20	53,28	—,39 *p*
Bar. 30,01	E	8 16,8	36 0,0	1 23,26	52,92	
Therm.	E	10 29,8	36 58,7	1 23,30	51,86	
Int. 43°						
Ext. 40°					52,90	
22	E	10 16,6	35 53,0	1 23,72	53,21	
	E	7 2,6	35 32,5	1 23,65	53,07	—,02 *x*
	E	3 25,6	34 38,3	1 23,61	53,21	
In.	W	5 43,4	33 40,7	1 23,63	53,76	—,39 *p*
Bar. 30,13	W	8 19,4	34 34,2	1 23,67	54,83	
Therm.	W	11 53,4	36 16,6	1 23,76	53,75	
Int. 42°½						
Ext. 40°					53,64	
24	W	3 6,1	33 8,6	1 22,12	53,25	
	W	0 40,1	32 54,4	1 22,10	52,21	—,03 *x*
	W	0 49,9	32 55,0	1 22,10	52,46	
In.	E	9 43,9	36 37,1	1 22,15	51,20	—,39 *p*
Bar. 29,82	E	11 8,9	37 21,1	1 22,27	52,82	
Therm.	E	13 9,9	38 33,2	1 22,31	54,56	
Int. 46°						
Ext. 44°					52,75	

	Face of Circle.	Time from Meridian.	Bottom Microscope.	Refraction.	Mean Z D. Jan. 1, 1819.	
1818.		′ ″	° ′ ″	′ ″	° ′ ″	
Dec. 28	E	3 27,2	54 34 39,2	1 24,07	54 34 53,91	
	E	1 0,2	34 23,6	1 24,07	53,99	—,06 *x*
In.	W	6 50,8	34 0,3	1 24,11	53,13	—,39 *p*
Bar. 30,37	W	9 3,8	34 49,5	1 24,15	51,72	
Therm.	W	12 8,8	36 22,3	1 24,23	50,60	
Int. 43°½						
Ext. 38°					52,67	
1819.						
Aug. 17	W	11 40,2	35 51,5	1 19,83	49,62	
	W	8 18,2	34 15,0	1 19,77	49,26	+,33 *x*
	W	6 27,2	33 36,7	1 19,72	49,88	
	W	4 22,2	33 6,1	1 19,72	51,67	+,22 *p*
	E	6 10,8	34 56,7	1 19,72	51,44	
In.	E	8 19,8	35 41,5	1 19,77	51,66	
Bar. 30,13	E	9 58,8	36 23,9	1 19,80	50,89	
Therm.	E	11 49,8	37 23,5	1 19,83	52,82	
Int. 65°						
Ext. 57°					50,90	
18	W	7 59,9	34 10,5	1 19,89	52,01	
	W	4 57,9	33 16,3	1 19,83	54,10	+,33 *x*
	W	3 2,9	32 52,4	1 19,81	52,25	
In.	E	6 18,1	34 59,2	1 19,83	52,07	+,21 *p*
Bar. 30,06	E	8 17,1	35 40,8	1 19,89	52,23	
Therm.	E	10 12,1	36 32,0	1 19,93	52,82	
Int. 65°						
Ext. 59°					52,58	
20	E	9 48,0	36 20,6	1 19,67	52,49	
	E	7 19,0	35 18,8	1 19,62	51,73	+,34 *x*
	E	4 54,[illegible]	34 37,1	1 19,58	52,29	
In.	W	6 59,0	33 49,9	1 19,58	53,04	+,20 *p*
Bar. 30,01	W	9 18,0	34 43,1	1 19,62	52,04	
Therm.	W	12 37,0	36 28,8	1 19,72	53,44	
Int. 63°½						
Ext. 58°					52,50	
21	E	10 12,0	35 6,1	1 19,62	49,95	
	E	8 13,0	34 14,3	1 19,56	50,59	+,34 *x*
	E	6 8,0	33 31,1	1 19,46	50,24	
	E	4 17,0	33 3,9	1 19,46	50,73	+,19 *p*
	W	5 0,0	34 39,4	1 19,51	53,32	
In.	W	7 4,0	35 14,5	1 19,56	52,64	
Bar. 29,96	W	8 48,0	35 53,4	1 19,56	51,99	
Therm.	W	11 58,0	37 26,9	1 19,67	51,26	
Int. 63°½						
Ext. 57°					51,34	
22	E	8 50,4	35 55,5	1 19,46	53,10	
	E	7 12,4	35 17,4	1 19,46	53 00	+,35 *x*
	E	5 26,4	34 45,7	1 19,41	53 03	
	E	3 23,4	34 20,9	1 19,41	54,24	+,18 *p*
	W	7 59,6	34 11,5	1 19,46	53,00	
In.	W	9 49,6	34 57,8	1 19,51	52,45	
Bar. 29,95	W	12 13,6	36 12,5	1 19,56	51,10	
Therm.	W	14 35,6	37 43,8	1 19,61	51,25	
Int. 64°						
Ext. 58°					52,65	

TABLE VIII. *continued.*

	Face of Circle.	Time from Meridian.	Bottom Microscope.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1819.		′ ″	° ″ ′	′ ″	° ′ ″	
Sept. 10	W	8 17,0	54 34 15,7	1 20,40	54 34 52,89	
	W	6 35,0	33 40,5	1 20,34	53,94	+,38 *x*
	W	4 35,0	33 7,0	1 20,34	52,53	
In.	E	6 0,0	34 50,7	1 20,35	51,29	+,07 *p*
Bar. 29,92	E	7 56,0	35 29,0	1 20,40	50,94	
Therm.	E	9 27,0	36 6,0	1 20,40	50,07	
Int. 58°						
Ext. 51°½					51,94	
Sept. 12	W	11 6,8	35 34,7	1 21,27	53,98	
	W	9 15,8	34 40,0	1 21,23	53,41	+,39 *x*
	W	7 38,8	34 1,6	1 21,23	54,29	
	W	5 15,8	33 17,7	1 21,18	54,54	+,06 *p*
	E	4 37,2	34 28,0	1 21,18	50,58	
In.	E	6 12,2	34 53,5	1 21,18	51,45	
Bar. 30,03	E	7 56,2	35 28,5	1 21,23	51,29	
Therm.	E	9 27,2	36 6,2	1 21,23	51,10	
Int. 59°						
Ext. 55°					52,58	
14	E	9 47,2	36 18,5	1 20,10	53,18	
	E	7 53,2	35 29,5	1 20,07	52,36	+,39 *x*
	E	6 3,2	34 53,1	1 20,03	52,68	
	E	3 54,2	34 21,1	1 20,03	51,43	+,04 *p*
	W	5 0,6	33 12,4	1 20,03	51,96	
In.	W	7 9,8	33 50,2	1 20,03	52,10	
Bar. 29,90	W	9 51,8	34 55,4	1 20,10	51,33	
Therm.	W	12 22,8	36 15,0	1 20,16	50,56	
Int. 59°½						
Ext 54°					51,95	
16	W	8 5,2	34 9,6	1 21,50	52,87	
	W	5 53,0	33 23,3	1 21,46	50,75	+,39 *x*
	W	4 2,0	32 58,5	1 21,46	52,31	
	W	2 4,0	32 41,3	1 21,41	52,29	+,03 *p*
	E	6 1,0	34 50,0	1 21,46	51,77	
In.	E	8 26,0	35 41,8	1 21,50	53,44	
Bar. 29,87	E	10 14,0	36 29,8	1 21,55	53,22	
Therm.	E	12 2,0	37 26,1	1 21,61	51,99	
Int. 50°						
Ext. 43°					52,33	
Dec. 3	E	8 26,2	35 42,5	1 23,26	53,30	
	E	6 24,2	34 58,2	1 23,20	52,35	+,11 *x*
In.	W	10 16,8	35 5,7	1 23,30	52,31	
Bar. 29,86	W	11 31,8	35 45,8	1 23,30	53,28	−,37 *p*
Therm.	W	13 16,8	36 46,8	1 23,35	52,13	
Int. 40°½						
Ext. 37°					52,67	
4	E	7 55,7	35 33,2	1 22,14	54,83	+,10 *x*
In.	E	5 55,7	34 53,2	1 22,08	54,59	−,37 *p*
Bar. 29,58	E	4 32,7	34 31,0	1 22,08	53,20	
Therm.						
Int. 42°½						
Ext. 42°½					54,21	
8	W	6 6,0	33 28,5	1 24,24	54,23	
	W	4 28,0	33 3,0		53,38	+,08 *x*
In.	E	3 41,0	34 18,1		52,13	
Bar. 29,84	E	5 11,0	34 37,3		52,52	−,38 *p*
Therm.	E	8 41,0	35 47,0		52,50	
Int. 34°½						
Ext. 31					52,95	

	Face of Circle.	Time from Meridian.	Bottom Microscope.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1819.		′ ″	° ′ ″	′ ″	° ′ ″	
Dec. 14	E	1 53,9	54 34 7,2	1 22,86	54 34 54,02	+,04 *x*
	E	1 49,1	34 7,9	1 22,86	55,15	
	E	3 47,1	34 23,3	1 22,90	54,76	−,39 *p*
In.	W	10 50,1	35 24,6	1 22,97	53,35	
Bar. 29,27	W	13 56,6	37 14,9	1 23,09	53,26	
Therm.	W	15 3,6	38 2,0	1 23,13	53,93	
Int. 33°						
Ext. 32°					54,08	
15	W	6 49,0	33 42,6	1 22,82	52,94	+,03 *x*
	W	5 29,5	33 20,0	1 22,80	53,75	
	W	4 28,0	33 5,1	1 22,80	53,52	−,39 *p*
In.	E	6 3,0	34 54,7	1 22,80	53,97	
Bar. 29,31	E	7 24,0	35 20,5	1 22,82	53,71	
Therm.	E	8 30,5	35 45,7	1 22,85	53,60	
Int. 34°						
Ext. 33°					53,58	
23	E	4 58,5	34 37,9		52,53	
	E	3 38,0	34 22,1	1 21,68	53,32	−,03 *x*
	E	2 49,0	34 15,3		54,09	
In.	W	3 35,0	32 55,9		52,86	−,39 *p*
Bar. 29,16	W	5 16,0	33 17,6		53,16	
Therm.	W	6 53,5	33 45,6		52,77	
Int. 38°						
Ext. 34°					53,12	
28	E	4 36,3	34 34,3		55,27	
	E	3 2,3	34 16,3	1 23,28	54,48	−,06 *x*
	E	1 56,3	34 8,4		54,44	
In.	W	4 49,7	33 12,4		55,57	−,39 *p*
Bar. 29,41	W	6 8,7	33 32,8		55,21	
Therm.	W	7 48,7	34 5,3		54,29	
Int. 33°						
Ext. 32°					54,88	
1820.						
Sept. 1	W	6 39,0	33 20,0		53,73	
	W	5 25,5	32 58,1	1 20,75	52,94	+,37 *x*
	W	3 49,0	32 37,3		53,50	
In.	E	0 47,0	33 45,7		54,03	+,13 *p*
Bar. 29,90	E	2 48,0	33 55,3		53,24	
Therm.	E	4 21,0	34 12,3		54,32	
Int. 55°						
Ext. 48°½					53,63	
2	W	6 41,0	33 21,0		54,14	
	W	5 30,0	32 59,1	1 20,55	52,63	+,37 *x*
	W	3 52,0	32 37,3		52,82	+,12 *p*
In.	E	4 17,0	34 9,6		52,32	
Bar. 29,89	E	5 22,0	34 25,6		53,29	
Therm.	E	6 25,0	34 43,2		53,12	
Int. 56°						
Ext. 49°					53,05	
6	E	5 27,0	34 29,3		54,99	
	E	3 50,0	34 8,0	1 19,58	55,26	+,38 *x*
	E	1 28,0	33 50,2		55,48	+,10 *p*
In.	W	5 53,0	33 9,8		56,35	
Bar. 29,71	W	7 15,0	33 34,7		55,46	
Therm.	W	9 8,0	34 19,4		55,83	
Int. 59°						
Ext. 56°					55,56	

TABLE VIII. *continued.*

	Face of Circle.	Time from Meridian.	Bottom Microscope.	Refraction.	Mean Z. D. Jan. 1, 1819.	
		′ ″	° ′ ″	′ ″	° ′ ″	
1820. Sept. 8	E	5 45,5	54 34 31,0		54 34 52,92	
	E	4 10,0	34 7,6	1 20,65	52,22	+,38 *x*
	E	2 27,0	33 51,4		52,35	+,09 *p*
In.	W	4 47,0	32 48,5		53,11	
Bar. 30,05	W	7 13,0	33 29,0		51,65	
Therm.	W	8 44,0	34 4,5		52,39	
Int. 58°						
Ext. 54°					52,44	
10	E	2 47,4	33 54,3	1 19,99	52,14	+,38 *x*
	E	0 42,4	33 43,8		52,10	
	E	0 41,6	33 42,8		51,13	+,07 *p*
In.	W	5 50,6	33 4,3		52,18	
Bar. 30,01	W	7 39,6	33 39,4		52,03	
Therm.	W	10 47,1	35 1,0		50,81	
Int. 61°½						
Ext. 55°½					51,73	
11	W	6 11,4	33 12,0	1 19,51	53,46	+,39 *x*
	W	5 1,4	32 53,3		53,56	
	W	3 23,4	32 33,2		53,21	+,06 *p*
In.	E	3 13,6	34 0,7		54,34	
Bar. 29,87	E	4 34,1	34 14,1		52,71	
Therm.	E	6 21,6	34 42,7		53,18	
Int. 62°						
Ext. 58°					53,41	
17	E	8 14,4	35 19,8	1 20,29	51,91	+,39 *x*
	E	7 16,4	34 58,7		52,36	
	E	5 34,4	34 26,4		51,44	+,02 *p*
In.	W	0 12,6	32 14,0		51,51	
Bar. 29,61	W	2 1,6	32 20,3		51,99	
Therm.	W	3 28,1	32 31,3		51,61	
Int. 53°						
Ext. 44°					51,80	
Dec. 19	E	5 18,2	34 25,0	1 22,42	53,67	—,00 *x*
	E	4 5,2	34 8,3		53,39	
	E	3 7,2	33 58,3		53,40	—,39 *p*
In.	W	3 27,8	32 35,0		55,17	
Bar. 29,94	W	4 52,8	32 51,3		54,49	
Therm.	W	5 53,8	33 3,8		51,24	
Int. 46°						
Ext. 44°					53,56	
28	E	8 45,4	35 36,2	1 23,78	55,89	—,06 *x*
	E	2 25,4	33 53,8		55,20	
	E	0 36,4	33 44,5		53,81	—,39 *p*
In.	W	4 57,6	32 52,3		55,12	
Bar. 29,72	W	6 49,6	33 23,6		54,81	
Therm.	W	8 27,6	33 59,2		54,55	
Int. 35°						
Ext. 33°					54,90	

To show the consistency of the several observations of each day, they have been reduced, which otherwise would have been unnecessary, by applying the correction for collimation to each reading off. These corrections are for the *bottom* microscope, which was only used for α Aquarii. They were as follow:

		′	
1818.	Sept. 2, 4	41,39	—face East.
	Dec. 16, 28	44,15	+face West.
1819.	Aug. 17—Sept. 16	41,70	
	Dec. 3, 28	42,61	
1820.	Sept. 1, 17	43,96	
	Dec. 19, 28	44,16	

Corrections of collimation for γ and β Aquilæ for mean of *three* microscopes.

γ Aquilæ.		″	
1818.	July 14—Aug. 16	54,81	—face East.
	Oct. 16—Nov. 14	47,90	+face West.
	Nov. 20—Nov. 24	41,12	
	Dec. 7, 9	43,43	
1819.	Aug. 2—Sept. 7	47,04	
	Nov. 16, 29	47,25	
1820.	July 13, 25	48,20	
	Oct. 29 — Nov. 21	48,40	

β Aquilæ.		″	
1818.	July 17—Aug. 16	55,24	—face East.
	Oct. 16—Nov. 14	48,22	+face West.
	Nov. 20—Nov. 24	41,34	
1819.	Aug. 2—Sept. 7	47,10	
	Nov. 16, 26	47,51	
1820.	July 13, 25	48,53	
	Oct. 29 — Nov. 21	48,28	

TABLE IX. γ Aquilæ.

	Face of Circle.	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	x	p
1818.		° ′ ″		° ′ ″		
July 14	W	43 10 45,30	53,32	43 12 23,90	+,13	+,53
17	E	12 34,50	52,73	23,44	,16	,52
24	W	10 44,00	52,16	23,27	,22	,50
Aug. 6	E	12 30,37	53,44	23,46	,31	,44
9	W	10 39,80	53,78	23,31	,34	,43
10	E	12 27,40	53,69	21,36	,35	,42
11	W	10 39,57	53,86	23,49	,35	,42
12	E	12 27,77	53,75	22,09	,36	,41
13	W	10 38,20	53,80	22,34	,37	,40
14	E	12 29,87	53,74	24,44	,38	,39
16	E	12 30,07	53,68	24,84	,39	,38
1819. Aug. 2	W	10 41,70	52,55	23,19	,28	,46
4	E	12 15,33	52,82	23,33	,30	,45
7	W	10 39,83	52,88	22,44	,32	,44
9	E	12 14,33	53,01	23,30	,34	,43
11	W	10 39,67	52,60	22,63	,35	,42
12	E	12 16,20	52,48	25,12	,36	,41
15	W	10 39,27	53,01	23,21	,38	,39
18	E	12 14,23	53,00	24,46	,40	,37
20	W	10 39,50	52,82	23,90	,41	,35
21	E	12 13,60	52,96	24,19	,42	,34
22	W	10 38,57	52,83	23,25	,42	,34
24	E	12 13,73	52,85	24,60	,44	,32
Sept. 1	W	10 40,47	53,37	26,80	,47	,27
3	E	12 10,77	53,11	22,97	,48	,26
6	W	10 40,73	53,23	27,39	,49	,22
7	E	12 12,87	52,57	24,87	,49	,21

		Time from Meridian	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1820.		′ ″	° ′ ″	″	° ′ ″	
July 13	W	4 50,2	43 11 16,90	52,84	43 12 26,01	+,12 x
	E	3 17,8	12 29,20		23,00	+,53 p
15	E	3 23,8	12 30,43	52,93	23,59	+,14 x
	W	4 11,2	11 4,13		23,60	+,52 p
18	W	6 17,8	11 41,13	52,16	23,17	+,16 x
	E	0 11,8	12 12,10		24,43	+,51 p
19	E	3 5,8	12 28,13	52,40	24,79	+,17 x
	W	4 7,2	11 4,90		25,53	+,51 p
24	E	7 31,0	13 44,30	53,19	23,71	+,22 x
	W	2 10,5	10 40,90		23,87	+,50 p
25	W	7 33,5	12 8,37	53,46	23,56	+,23 x
	E	0 31,[illegible]	12 11,70		26,23	+,49 p

	Face of Circle.	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	x	p
1818.		° ′ ″	″	° ′ ″		
Oct. 16	W	43 10 44,00	53,44	43 12 24,80	+,52	−,14
17	E	12 19,80	53,68	25,01	,52	,15
21	W	10 43,43	53,70	24,35	,51	,19
Nov. 2	W	10 45,07	53,04	24,77	,46	,28
3	E	12 20,83	52,93	24,53	,46	,29
7	E	12 21,17	53,86	25,48	,44	,32
8	W	10 45,47	54,16	25,80	,43	,33
14	E	12 21,93	52,94	24,72	,39	,38
20	W	10 54,17	53,44	25,84	,35	,42
23	E	12 16,80	52,96	25,42	,32	,43
24	W	10 53,43	53,80	25,00	,32	,44
Dec. 7	W	10 54,60	53,43	26,38	,21	,50
8	E	12 20,47	54,54	26,35	,20	,50
9	W	10 53,17	55,08	26,31	,20	,51

		Time from Meridian	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1819.		′ ″	° ′ ″	″	° ′ ″	
Nov. 16	W	3 21,3	43 10 56,33	54,88	43 12 25,46	+,38 x
	E	4 3,7	12 39,17	54,88	24,97	−,39 p
22	W	5 9,1	11 23,30	55,16	26,35	+,33 x
	E	5 35,9	13 5,37	55,16	25,84	−,43 p
23	E	6 6,5	13 16,13	55,78	27,04	+,32 x
	W	1 57,5	10 43,80	55,78	25,58	−,43 p
24	W	9 44,0	13 15,23	56,30	24,35	+,32 x
	E	0 51,0	12 12,80	56,23	25,65	−,44 p
26	E	7 19,2	13 43,40	55,40	26,18	+,30 x
	W	0 52,8	10 39,37	55,32	25,49	−,45 p
27	W	5 12,4	11 23,60	55,53	25,52	+,30 x
	E	4 23,6	12 43,50	55,53	24,07	−45, p
29	E	4 6,2	12 42,07	54,20	25,15	+,28 x
	W	7 18,8	12 10,23	54,23	26,13	−,47 p
1820. Oct. 29	W	9 37,7	13 3,57	53,28	25,84	+,48 x
	E	1 32,2	12 7,83		25,35	−,25 p
Nov. 1	E	3 41,6	12 25,57	54,29	24,97	+,47 x
	W	3 6,4	10 43,10		26,02	−,28 p
2	E	2 23,6	12 12,47	54,45	25,31	+,46 x
	W	3 23,4	10 45,67		25,60	−,28 p
7	E	2 56,3	12 19,10	53,44	25,66	+,44 x
	W	4 17,7	10 59,33		26,17	−,32 p
11	W	1 44,4	10 30,97	55,31	25,34	+,41 x
	E	5 31,6	12 53,50		24,74	−,35 p
14	E	8 50,4	14 17,63	55,23	28,39	+,39 x
	W	3 4,4	10 44,13		27,33	−,38 p
15	E	3 47,7	12 27,47	55,55	25,75	+,39 x
	W	2 29,3	10 36,83		25,73	−,39 p
17	W	3 2,0	10 43,93	54,24	26,25	+,37 x
	E	3 23,0	12 25,17		26,91	−,39 p
21	W	3 43,2	10 53,00	53,02	25,89	+,34 x
	E	2 22,8	12 17,33		27,18	−,42 p

TABLE X. β Aquilæ.

	Face of Circle.	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	x	p
1818.		° ′ ″	′ ″	° ′ ″		
July 17	E	47 25 29,83	1 1,14	47 25 26,64	+,15	+,46
24	W	23 37,53	1 0,48	25,31	,21	,44
25	E	25 24,77	1 1,33	23,09	,21	,43
Aug. 6	E	25 22,13	1 1,95	22,83	,29	,39
9	W	23 32,53	1 2,33	24,48	,31	,37
10	E	25 23,90	1 2,23	25,41	,32	,36
11	W	23 32,73	1 2,43	25,04	,33	,36
12	E	25 22,40	1 2,30	24,24	,33	,35
13	W	23 32,97	1 2,36	25,47	,34	,35
14	E	25 24,43	1 2,29	26,49	,35	,34
16	E	25 24,63	1 2,22	26,84	,36	,33
1819.						
Aug. 2	W	23 33,80	1 0,92	23,60	,26	,40
4	E	25 9,33	1 1,30	25,58	,28	,40
9	E	25 8,30	1 1,46	25,40	,31	,37
11	W	23 33,20	1 0,98	24,29	,33	,36
15	W	23 33,23	1 1,46	25,29	,35	,34
18	E	25 8,47	1 1,45	26,65	,37	,32
20	W	23 32,97	1 1,23	25,37	,38	,30
21	E	25 8,37	1 1,40	26,85	,39	,29
22	W	23 31,60	1 1,25	24,25	,39	,29
Sept. 1	W	23 34,70	1 1,87	28,94	,43	,22
3	E	25 4,00	1 1,57	23,92	,44	,21
6	W	23 34,10	1 1,72	28,58	,45	,19
7	E	25 6,57	1 0,95	26,15	,45	,18

		Time from Meridian	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1820.		′ ″	° ′ ″	′ ″	° ′ ″	
July 13	E	0 4,7	47 25 7,13	1 1,22	47 25 27,81	+,12 x
	W	7 26,7	24 57,17		27,23	+,47 p
15	W	1 5,6	23 30,00	1 1,34	26,32	+,14 x
	E	5 24,9	25 50,50		25,27	+,46 p
18	E	5 12,0	25 48,57	1 0,45	26,57	+,16 x
	W	0 53,0	23 30,07		26,67	+,45 p
19	W	1 43,0	23 33,60	1 0,82	27,31	+,17 x
	E	4 54,0	25 43,53		26,86	+,45 p
24	W	6 31,1	24 32,90	1 1,64	25,73	+,21 x
	E	0 23,9	25 3,53		26,26	+,44 p
25	E	5 6,1	25 44,87	1 1,95	27,15	+,21 x
	W	0 30,9	23 26,50		26,59	+,43 p

	Face of Circle.	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	x	p
1818.		° ′ ″	′ ″	° ′ ″		
Oct. 16	W	47 23 37,93	1 1,94	47 25 26,24	+,46	—,14
17	E	25 13,17	1 2,22	25,29	,46	,15
21	W	23 36,73	1 2,25	25,21	,45	,18
Nov. 1	E	25 16,73	1 2,17	28,20	,41	,26
2	W	23 38,53	1 1,48	25,70	,40	,26
3	E	25 14,57	1 1,35	25,11	,40	,27
7	E	25 15,17	1 2,43	26,50	,38	,30
8	W	23 39,97	1 2,78	28,02	,38	,31
14	E	25 16,63	1 1,36	26,35	,34	,35
20	W	23 48,57	1 1,95	27,88	,29	,39
23	E	25 11,53	1 1,39	27,30	,27	,40
24	W	23 47,40	1 2,36	26,71	,27	,41

		Time from Meridian	Mean of three Microscopes.	Refraction.	Mean Z. D. Jan. 1, 1819.	
1819.		′ ″	° ′ ″	′ ″	° ′ ″	
Nov. 16	E	0 34,5	47 25 6,53	1 3,59	47 25 27,20	+,33 x
	W	6 16,5	24 35,27	1 3,63	29,23	—,36 p
22	E	2 23,7	25 17,27	1 3,95	29,22	+,28 x
	W	8 28,7	25 25,60	1 3,98	27,95	— 40 p
23	W	0 0,0	23 31,70	1 4,62	28,30	+,27 x
	E	12 39,4	29 18,80	1 4,82	27,29	—,40 p
24	E	1 56,0	25 11,90	1 5,15	27,98	+,27 x
	W	6 32,0	24 36,00	1 5,19	25,52	—,41 p
26	W	1 1,4	23 33,60	1 4,13	27,70	+,25 x
	E	8 12,6	26 52,80	1 4,21	26,99	—,41 p
1820.						
Oct. 29	E	4 22,5	25 28,07	1 1,75	27,13	+,42 x
	W	4 54,0	23 59,60		27,52	—,24 p
Nov. 1	W	1 53,8	23 26,60	1 2,93	27,86	+,41 x
	E	5 10,2	25 38,83		26,94	—,26 p
2	W	0 48,3	23 21,73	1 3,12	27,81	+,40 x
	E	6 15,2	25 57,80		26,49	—,26 p
7	W	0 48,5	23 23,27	1 1,95	27,84	+,38 x
	E	4 37,5	25 33,63		28,84	—,30 p
11	E	0 51,4	24 57,77	1 4,13	27,58	+,36 x
	W	7 50,4	24 56,53		26,83	—,33 p
15	W	2 57,9	23 34,40	1 4,37	27,93	+,34 x
	E	2 21,1	25 6,20		28,33	—,35 p
17	E	1 58,7	25 6,87	1 2,90	29,90	+,32 x
	W	5 31,3	24 11,83		29,38	—,36 p
21	E	2 32,5	25 12,07	1 1,45	29,27	+,29 x
	W	3 10,5	23 41,27		29,30	—,39 p

XXV. *On the effects produced in the rates of Chronometers by the proximity of masses of iron. By* PETER BARLOW, *Esq. of the Royal Military Academy. Communicated by* JOHN BARROW, *Esq. F. R. S.*

Read June 28, 1821.

IT having been ascertained during the voyage made by Captain BUCHAN to the Arctic regions, in the year 1818, that the rates of the chronometers were considerably different on board and on shore, and this change having been attributed to the iron of the vessel,* I felt very desirous, first, of ascertaining whether the proximity of a mass of iron had actually any effect in changing the rate; and, secondly, supposing this to be the case, to determine, if possible, the laws and principles by which that action was governed.

I accordingly, through the kindness of some of my friends, procured the loan of six excellent chronometers, besides one or two others, which upon trial were found to have too wide and irregular rates for my purpose. Having procured these, and made the requisite preparations, I began my series of observations on them on the 11th of March of the present year, and continued them daily till the 25th of May; when, having obtained a considerable number of results, they were discontinued.† It will however be proper, before I proceed

* See a Memoir by Mr. FISHER, who accompanied Captain BUCHAN, in the Philosophical Transactions for 1820, Part II.

† I ought not to omit this opportunity of returning my best thanks to those

to the detail of particulars, to explain the views I had formed on the subject, and the principles upon which I conducted the experiments.

I conceived, that if such an effect as that described by Mr. Fisher, were generally produced on the rates of watches and chronometers, it must arise from the spring, or some part of the balance having become magnetic, and the consequent attraction of the iron upon it. But this would lead us also to conclude, that accordingly as the balance was placed in this, or that direction, with respect to any given mass of iron, the rate of the chronometer would be accelerated or retarded, and not uniformly accelerated, as would seem to be the case by Mr. Fisher's observations. Or rather perhaps I ought to say, that a different direction of the balance would alter the arc of its vibration, from greater to less, or from less to

friends who have, in these experiments, favoured me with their advice and assistance. To my late colleague, the Reverend Mr. Evans, I am much indebted for the loan of his gold pocket chronometer by Earnshaw, and by his procuring for me, through the kindness of Mr. Pennington, an excellent box chronometer by that Gentleman, marked No. 4 in the following series. To my friend Captain Lynn I am under an equal obligation, by his having entrusted to my care his very fine box chronometer, marked No. 3, and by his having procured for me, from Mr. Arnold, the silver pocket chronometer No. 2. These two were selected out of a great number which the former Gentleman was employed in rating, in consequence of their being decidedly the most uniform in their action. To Captain Colby and to Mr. Arthur Baily, I am indebted for my introduction to Messrs. Parkinson and Frodsham, who in the most liberal and handsome manner furnished me with the two chronometers No. 5 and No. 6. The latter of these is adjusted according to the new principle of these makers, and was made at the same time, and is in all respects similar to their chronometers No. 228, 253, 254, and 259, which were so much distinguished in Captain Parry's late voyage. To these Gentlemen I am also much indebted for the means of making the experiments, reported in a subsequent page, on the detached chronometrical parts of such a machine.

greater; but it would still depend upon the original adjustment of the machine, whether the result would be to accelerate or to retard its action; that is to say, it would depend upon the contingency, whether the chronometer had a tendency to gain, or lose, in short arcs, which I am informed is nearly an equal chance, if it proceed from the hands of a scientific workman; but that, in general cases, the probability is, that the watch will lose in large arcs, and gain in small ones.

The experiments and observations which Mr. FISHER describes as having been made with a strong bar magnet, brought within two inches of the balance, I consider to be perfectly distinct in their nature from those which were made by him on board and on shore at Spitzbergen; for a magnet of such power, brought within the distance of two inches of any small piece of steel, will, whether the latter be previously magnetic or not, impress upon it a strong temporary derangement of its latent magnetism, and give to the part nearest the magnet, a contrary pole to that by which it is opposed; and consequently, there will exist between the balance and the magnet a strong power of attraction sufficient to cause that acceleration so strongly indicated in Mr. FISHER's experiments; and this will be the case whichever end of the magnet is opposed to the balance, and to whatever part of the latter the application is made; because, in this instance, the effect does not depend upon the previous magnetic state of the balance, but upon that temporary state excited by the proximity of the magnetic bar, and which ceases when the bar is removed.

This explanation will not, I conceive, apply to the action of

plain unmagnetized iron; for notwithstanding, according to the present received doctrine of magnetism, every mass of soft iron becomes a temporary magnet by induction from the earth; yet I am not aware that ever any particular action has been discovered between two pieces of iron, whether hard or soft, which had not previously acquired a polar quality; the largest mass of iron, for instance, will not, that I am aware of, attract and give direction to the lightest and most freely suspended needle of soft iron, or of unmagnetized steel.

Now, if this be admitted, it necessarily follows, that plain unmagnetized iron can only be supposed to act on the balance of a chronometer, when that balance has acquired a polar or directive quality; and then, as I have already stated, it will have a tendency to produce an acceleration, or retardation, according to its position with respect to the balance, and the previous adjustment of the machine.

If this be actually the case, it may probably appear singular, that all Mr. Fisher's chronometers were accelerated; but it is not much less so, that five out of the six which I used in my experiments were as decidedly retarded. It will likewise, after examining my experiments, be difficult to account for that high degree of acceleration noticed by Mr. Fisher; for it will be seen that, although I approximated some of my chronometers to within two or three inches of the surface of an iron ball thirteen inches in diameter, the utmost effect which I could produce did not exceed 4″ per day; whereas Mr. Fisher makes *his* amount to 8″ or 9″ per day; and yet we can scarcely imagine that he brought his chronometers so closely within the immediate sphere of action of any mass of iron, more powerful than that described in

my experiments; indeed we are led strongly to suspect, that the remarkable change in the rates of the nine chronometers of the Dorothea and Trent, reported by Mr. FISHER, must have been produced by some extraordinary cause, not commonly operating on ship board.

I have already observed, that, according to the idea I entertain of the action of iron on the balance of a chronometer, it is actually necessary to conceive, that part of the machine, or at least its spring, to have acquired a certain polar or directive quality, whereby, independent of any other power, the balance would have a tendency to assume a certain direction, when brought within the sphere of action of a given mass of iron; and the amount of that tendency might, I conceived, be estimated, by counting the number of vibrations which a small magnetized needle would make in a given time, in any assigned situation, near the iron, and comparing the result with the number it would make under like circumstances, and in the same time, when wholly removed from any attracting mass.

In order to illustrate this view of the subject a little more particularly, let A B C D (fig. 1. Pl. XXV.) represent the balance of a chronometer, s, s' its spring, and let D be that part of the rim which is attracted by the centre o, of an iron ball or shell. If now we conceive the spring to be detached from the fixed part of the machine, it will be free, with the balance itself, to take any position. The point D will therefore be attracted towards o; and if it be displaced from this position, it will have a tendency to oscillate on each side of the point D; and the number of vibrations which it would make in a given

time would serve, if we could obtain such results, to estimate the intensity of action of the attracting body.

But although we cannot detach the balance for such an experiment, we may still form some idea of the intensity of action, by causing a small magnetized needle to oscillate in the place of the balance, and by counting the number of its vibrations as above described. Indeed there is not much difficulty in estimating, theoretically, the change of intensity due to a certain change in the position and distance of the attracting body ; but I prefer experiment, as more satisfactory to those who may not be able to follow out completely the mathematical investigation on which such a computation must depend. With this previous view of the subject, I began with first ascertaining the time in which forty vibrations were made with a small magnetic needle in different situations with respect to an iron shell eighteen inches in diameter, and at eighteen inches distance from its centre ; the weight of the shell being 496 lbs.

But as the degree of intensity, as well as the quantity of deviation, occasioned by the iron ball, has reference, not to the plane of the horizon, but to *the plane of no attraction*,* I proceeded with these experiments as follows :

Let S Q N Q′ (fig. 2. Pl. XXV.) represent the iron shell, or a sphere concentric with it ; Q Q′ its magnetic equator, or plane of no attraction, and *ab*, *cd*, *ef*, &c. parallels of latitude answering to 60°, 45°, 30°, &c. H H′ the horizon, and S N the natural direction of the magnetic action in this place ; the circle S Q N Q′ denoting the plane of the magnetic me-

* See "Essay on Magnetic Attraction," page 18.

ridian, agreeably to the division of the magnetic sphere, as described in my "Essay on Magnetic Attraction."

I now first placed the compass at Q, eighteen inches from the centre of the shell, and observed the number of seconds which the needle employed to make forty vibrations; then, still keeping the needle in the circle QQ′, I placed it 30° from Q towards E, or in longitude 60°. I then brought it 30° nearer to E, or into longitude 30°; then to E, or longitude 0°; and so on at every 30° through the whole circle. The same was then repeated in the circles *ab*, *cd*, *ef*, &c.; and by taking the mean of the results for each corresponding situation on each side of the meridian, I obtained the numbers given in the following table:

TABLE,

Showing the time of making ten vibrations with a fine magnetic needle in different situations, eighteen inches from the centre of an iron shell, eighteen inches in diameter, weighing 496 lbs.

Latitude.	Mean time of making 10 vibrations.						
	Long. 90° N	Long. 60° N	Long. 30° N	Long. 0°	Long. 30° S	Long. 60° S	Long. 90° S
°	″	″	″	″	″	″	″
90 N	30·25						
60 N	26·00	27·25	28·00	29·75	34·50	35·75	38·25
45 N	26·25	27·50	28·25	30·00	35·25	39·25	43·50
30 N	27·25	27·50	29·00	31·25	34·00	41·25	46·50
0	34·50	34·75	34·75	35·50	35·00	35·00	35·00
30 S	46·50	41·25	34·00	31·25	29·00	27·50	27·25
45 S	43·50	39·25	35·25	30·00	28·25	27·50	26·25
60 S	38·25	35·75	34·50	29·75	28.00	27·25	26·00
90 S							30·25
Mean time of making 10 vibrations detached from the iron ball 32″· 50.							

These experiments were made with a small steel bar or magnetic needle, finely suspended with untwisted silk in a glass vessel, and some care was taken to get the time as accurately as seemed desirable for the purpose ; but as the only intention of the experiments was to have some general ideas of those situations near the ball, where a compass needle would be the most affected in its vibrations, and where also, according to my ideas, the chronometer would be most affected in its rate, I did not conceive it necessary to carry these observations to the utmost degree of precision.

Every thing being thus prepared, I applied to my friend the Reverend Mr. Evans, to allow the experiments to be conducted at his observatory, in which was an excellent transit instrument by Troughton, and every thing requisite for conducting them with the greatest accuracy. To this request he very readily assented ; and he superintended the observations with the utmost attention, from March 11 to April 30, when, being about to remove to another part of the country, he was obliged to dismantle his observatory, and the experiments, during the rest of the period, were carried on in the same way by myself, in the Observatory of the Royal Military Academy.

Explanation of the table of experiments.

In the *first* column is given the day of the month, and in the *second*, the state of the thermometer for each day at ten o'clock A. M.

The *third* column shows the rate of the observatory clock, as deduced from each two consecutive transit observations : it is of no other use than that of showing the degree of con-

fidence which is due to the daily rates of the chronometers on those days on which the sun's transit could not be taken.

The *fourth* column gives the quantity which each chronometer was fast or slow of mean time every day at noon, and from which is drawn the daily rate indicated in the *fifth* column.

The *sixth* column shows the mean daily rate for each period while the chronometers remained in the same position; and in the *seventh*, is shown the gain or loss in each position; it is found by taking the difference between the actual observed daily rate, and the mean detached rate. By the *mean detached rate*, is to be understood the mean rate on all those days when the chronometers were not applied to the ball.

In order to ascertain whether any law subsisted between the gain or loss of the watch and the magnetic intensity of the place in which it was situated, the needle, described in a preceding page, was vibrated in every situation where a chronometer had stood, and the mean time of its making ten vibrations carefully noted and entered in the *eighth* column; and in the *ninth* is given the proportional magnetic intensity, assuming that due to the natural state of the needle at 100.

In the *tenth* and last column, is described the particular situation of each chronometer, *viz.* its azimuth, height from the floor, and distance from the centre of the ball. These situations are also reduced to their particular latitudes, longitudes, and central distances, as referred to the ideal sphere circumscribing the ball, and explained in a preceding page. I have also, in every case, noted the direction of the chronometer itself, by stating whether the 12 o'clock mark on the dial pointed to the north, south, east, or west.

The plate and pedestal mentioned in two instances, are the same as those described in my " Essay on Magnetic Attraction," page 87. See also fig. 3. Pl. XXV. of this Memoir. The plate was double, one foot in diameter, and weighed about 5 lbs. It was placed vertically, and at the distance of ten inches from the vertical through the centre of the dial, and its centre ten inches below that of the chronometer.

At the distance of from twelve to fourteen inches from such a plate, its action is equal to the mean effect of all the iron of a vessel of medium rate, at least on the compass ; as I have ascertained by my experiments on board His Majesty's ship Leven and Conway ; and as farther appears from the observations of Captain Ross in the Isabella, and of Captain PARRY in the Hecla. I had, therefore, intended to make farther observations on the effect of this plate, had it not been rather unexpectedly called away to be fitted on board the Fury, with a view of ascertaining its efficacy in correcting the local attraction of that vessel in her present interesting voyage.

TABLE I.

Experiments and observations on the rates of Chronometers in the vicinity of iron bodies, at the Observatory of the Rev. Mr. Evans, *Woolwich Common. Lat.* 51° 29′ 8″ *North, long.* 4′ 10″ *East.*

No. I. *Pocket Chronometer in Gold Cases, by* Earnshaw. *Detached rate* —3″·2.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten Compass Vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
From Mar. 1 to 12.	°	......	....	—3·2	3·2	″ 0·0	....	....	These rates were taken prior to the experiments by Mr. Evans.
12	50	—1·4	″ +48·4	—3·2	—3·2	0·0	″ 32·5	100	These rates were taken before the Chronometer was applied to the ball.
13	51	......	+45·2	—3·2					
14	49	—1·1	+41·1	—4·1					
15	47	—1·5	+39·0	—2·1					
16	46	......	....		—5·6	—2·5	34·0	91	Chronometer to the South of the ball; 2·1 inches from the floor, and distant from the vertical, passing through the centre of the ball, 17·31 inches, corresponding to lat. 0°, long. 90°, and distance from centre 18 inches. 12 o'clock, South.
17	47	—0·9	+32·0	—3·5					
18	48	—1·05	+28·1	—3·9					
19	46	—0·9	+20·9	—7·2					
20	47	—1·05	+12·8	—8·1					
21	47	......	+ 5·0	—7·8					

This Chronometer was detached from the ball on the 21st, and its rate taken for a few days; but as it was very irregular, our observations on it were discontinued after the 24th. The mean rate of this Chronometer is assumed —3″·2.

TABLE II.

Experiments and observations on the rates of Chronometers in the vicinity of iron bodies, at the Observatory of the Rev. Mr. EVANS, *Woolwich Common. Lat.* 51° 29′ 8″ *North, long.* 4′ 10″ *East.*

No. II. *Pocket Chronometer, in Silver Cases, by* ARNOLD.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten compass vibrations.	Proportional Magnetic intensities.	Position of Chronometer, Remarks, &c.
Mar. 1st. to Mar. 11th	- -	- - -	- - - -	+6″·0	+6″·0	- - - -	- - -	- - -	These rates were taken by Captain LYNN prior to the experiments.
12	50°	—1·4	+2′ 32″·1		+5·0	0·0	32·5	100	These rates were taken at Woolwich before the Chronometer was applied to the ball.
13	51	- - -	+2 37·0	+4·9					
14	49	—1·1	+2 42·1	+5·1					
15	47	—1·5	+2 47·1	+5·0					
16	46	- - -	- - - -	- - -	+6·5	+1·5	30·0	117	Chronometer placed above the ball; height from floor 23 inches, distance from vertical through the centre 6 inches, to the South; corresponding to lat. 90° South, and distance 18 inches: 12 o'clock, North.
17	47	—0·9	+2 59·6	+6·2					
18	48	—1·05	+3 6·5	+6·9					
19	46	—0·9	+3 13·1	+6·6					
20	47	—1·05	+3 19·8	+6·7					
21	47	- - -	+3 26·3	+6·5					
22	48	- - -	+3 32·1	+5·8	+5·8	+0·8	32·5	100	Detached from the ball.
23	45½	—1·4	+3 37·3	+5·8					
24	47	—1·4	+2 7·5	- - -					
25	48	- - -	+2 13·0	+5·5	+5·2	+0·2	32·5	100	The Chronometer was placed in a new situation on the 23rd; but in consequence of its being suffered to go down on the 23rd at night, it was kept detached till the 30th, to be re-rated.
26	47	- - -	+2 18·2	+5·2					
27	49	—2·4	+2 24·1	+5·9					
28	48	- - -	+2 28·9	+4·8					
29	50	- - -	- - - - -	- - -					
30	48	—2·4	+2 39·1	+5·1					
31	49	- - -	+2 44·8	+5·7	+6·1	+0·9	33·5	94	Placed to the N. of ball; height 10½ inches; dist. from vertical 11·31, corresponding to lat. 0° long. 90°, dist. from centre 12 in. 12 o'clock S.
April 1	48	—2·5	+2 51·0	+6·2					
2	51	—1·7	+2 57·3	+6·3					
3	50	- - -	+2 38	- - -	+4·7	—0·5	32·5	100	Detached again from the ball.
4	49	—1·9	+2 42·8	+4·8					
5	49	—2·5	+2 47·3	+4·5					
6	47	- - -	+2 52·8	+5·5	+5·0	—0·2	25·5	162	Placed above the ball; height from floor 17·3 inches; distance from vertical 4 inches, to the South; corresponding to latitude 90° South, and distance from centre 12 inches. 12 o'clock South.
7	50	- - -	+2 57·4	+4·6					
8	- -	—1·9	+3 3·9	+6·5					
9	57	—1·9	+3 7·7	+3·8					
10	58	—1·7	+3 12·6	+4·9					
11	57	- - -	+3 16·8	+4·2	+4·0	—1·2	30·0	117	Same latitude, but at the distance of 18 inches, as on the 16th March. 12 o'clock, South.
12	55	—2·0	+3 20·5	+3·7					
13	53	- - -	+3 25·8	+5·3	+4·3	—0·9	32·5	100	As this chronometer appeared to be increasing its rate, or rather decreasing its losing rate, independent of the action of the iron, it was detached during these days, and our observations with it discontinued.
14	52	- - -	+3 29·9	+4·1					
15	50	- - -	+3 34·1	+4·2					
16	49	- - -	+3 38·6	+4·5					
17	49	—1·9	+3 42·4	+3·8					
18	51	—2·1	+3 46·3	+3·9					
19	53	- - -	+3 50·4	+4·1					

The numbers in column 7 are drawn from comparison with each preceding detached rate, and not from the mean detached rate, as is done in the following Tables.

TABLE III.

Experiments and observations on the rates of Chronometers in the vicinity of iron bodies, at the Observatory of the Rev. Mr. Evans, *Woolwich Common. Lat.* 51° 29′ 8″ *North, long.* 4′ 10″ *East.*

No. III. *Box Chronometer by* Barraud (*No.* 749) *Mean detached rate* +0.6.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten compass vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
Mar. 1st. to	- -	- - -	- - - -	″ +0·3	″ +0·3	- - - -	- - -	100	These rates were taken by Captain Lynn, at his observatory in town, prior to the Experiments.
Mar. 11th.	- -	- - -	- - - -						
12	° 50	—1·4	′ ″ +1 15·1	- - -	—0·4	- - - -	32·5	100	These rates were taken at Woolwich, before the Chronometer was applied to the ball.
13	51	- - -	+1 14·9	—0·2					
14	49	—1·1	+1 14·4	—0·5					
15	47	—1·5	+1 13·9	—0·5					
16	46	- - -	- - - -	—0·9	—0·9	—1·5	29·0	126	Chronometer placed to the south of the ball; height from the floor 11·3 inches; distance from vertical through the centre of the ball 17·3 inches, corresponding to lat. 35° 16′ S. long. 90°, and distance from centre 18 inches. 12 o'clock, South.
17	47	—0·9	+1 12·1	—0·9					
18	48	—1·05	+1 11·7	—0·4					
19	46	—0·9	+1 10·9	—0·8					
20	47	—1·05	+1 10·1	—0·8					
21	47	- - -	+1 8·4	—1·7					
22	48	- - -	+1 7·5	—0·9					
23	45½	—1·4	+1 6·8	—0·7	—0·9	—1·5	30·0	117	Placed above the ball; height from floor 23 inches; distance from vertical through the centre of the ball 6 inches, to the South; corresponding to the lat. 90° S. and central distance 18 inches. 12 o'clock, South.
24	47	—1·4	+1 6·3	—0·5					
25	48	- - -	+1 4·9	—1·4					
26	47	- - -	+1 3·9	—1·0					
27	49	—2·4	+1 3·2	—0·7					
28	48	- - -	+1 2·0	—1·2					
29	50	- - -	- - - -	- - -					
30	48	—2·4	+1 0·2	—0·9					
31	49	- - -	+0 59·6	—0·6	—0·2	—0·8	25.5	162	Same situation as the above, but the distance reduced to 12 inches from the centre. 12 o'clock, South.
April 1	48	—2·5	+0 59·0	—0·6					
2	51	—1·7	+0 59·3	+0·3					
3	50	- - -	+0 59·5	+0·2					
4	49	—1·9	+0 58·3	—1·2	—0·9	—1·5	35.5	84	Placed to the East of the ball; height 6·5 inches; distance from vertical 12 inches, or lat. 0° long. 0°. distance 12 inches. 12 o'clock South.
5	49	—2·5	+0 57·7	—0·6					
6	47	- - -	+0 56·7	—1·0					

This Chronometer being so little affected by the action of the iron, our observations on it were discontinued after the 6th, on which day it was returned to town; and by comparison with Greenwich time by Captain Lynn, on the 28th of April, its rate was found to have been 1″·0 per day gaining, which makes the mean rate, as stated above, +0·6.

TABLE IV.

Experiments and observations on the rates of Chronometers in the vicinity of iron bodies, at the Observat of the Rev. Mr. EVANS, *Woolwich Common. Lat.* 51° 29′ 8″ *North, long.* 4′ 10″ *East.*

No. IV. *Box Chronometer in Glass Case, by* PENNINGTON. *Detached rate* + 1″ ·5.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each Position.	Gain or Loss per day in each position.	Time of ten Compass Vibrations.	Proportional Magnetic Intensity.	Position of Chronometer, Remarks, &c.
	°		′ ″	″					
March 24	47	—1·4	—0 25·5		—0·5	—2·0	29·0	126	Placed the Chronometer to the South of ball; height from the floor 11·4 inches; tance from the vertical passing through centre of the ball 17·3 inches, correspond to lat. 35°16′ S, long. 90°, and central tance 18 inches. 12 o'clock, South.
25	48	……	—0 24·7	+0·8					
26	47	……	—0 26·0	—1·3					
27	49	—2·4	—0 26·4	—0·4					
28	48	……	—0 26·8	—0 4					
29	50	……	………	—0·9					
30	48	—2·4	—0 28·6	—0·9					
31	49	……	—0 27·0	+1·6	+2·1	……	32·5	100	Detached from the ball in order to obtain natural rate.
April 1	48	—2·5	—0 25·5	+1·5					
2	51	—1·7	—0 22·6	+2·9					
3	50	……	—0 20·3	+2·3					
4	49	—1·9	—0 20·8	—0 5	—0·3	—1·8	33·5	94	Placed to N. of ball; height 10·5 inches; dista from vertical 11·3 inches; or lat. 0°; long. central distance 12 inches. 12 o'clock, Sou
5	49	—2·5	—0 20·9	—0 1					
6	47	……	—0 21·1	—0 2					
7	50	……	—0 20·8	+0 3	+0·3	—1·2	33·5	94	Same situation. 12 o'clock, West.
8	……	—1·9	—0 19·6	+1·2					
9	57	—1·9	—0 20 3	—0·7					
10	58	—1·7	—0 19·5	+0·8	+0·8	—0·7	33·5	94	Same situation. 12 o'clock, East.
11	57	……	—0 19 2	+0·3					
12	55	—1·7	—0 18·0	+1·2					
13	53	—2·0	—0 16·6	+1·4	+0·6	—0·9	33·5	94	Same situation. 12 o'clock, North.
14	52	……	—0 16·1	+0·5					
15	50	……	—0 15·6	+0 5					
16	49	……	—0 15·7	—0·1					
17	49	—1·9	—0 15·1	+0 6					
18	51	—2·1	—0 14·9	+0·2	—0·6	—2·1	33·5	94	Same situation, but with the 12 o'clock, Sou as on the 4th, 5th, and 6th.
19	53	……	—0 14·4	+0·5					
20	55	……	—0 15·0	—0·6					
21	57	……	—0 16·4	—1·4					
22	56	—2·4	—0 17·9	—1·5					
23	56	—2·4	—0 18·1	—0·2	+0·2	—1·3	……*	……	Placed this Chronometer on pedestal, Sou of the plate; distance from vertical throu the centre of plate 10 inches; height abo centre 10 inches. 12 o'clock, South. S Description of plate, page 365.
24	60	—1·5	—0 17·7	+0·4					
25	63	……	—0 17·2	+0·5					
26	65	—0·8	—0 14·8	+2·4	+1·3	……	32·5	100	The Chronometer was detached during the days, to ascertain whether it would return its former detached rate.
27	63	—0·3	—0 13·5	+1·3					
28	62	—0·5	—0 12·4	+1·1					
29	60	—0 9	—0 11·6	+0·8					
30	56	……	—0 10·9	+0·7					

* Not taken for the reason assigned in page 376.

Observations on No. IV. *continued at the Royal Military Academy.*

Days.	Ther-mo-meter.	Clock Rate.	Chronometer + or — at Noon	Daily Rate of Chrono-meter.	Mean Rate in each position.	Gain or Loss per day in each po-sition.	Time of ten Compass Vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
rom Ap. 30 to May 6.	. . . . . .	* +1·06	....	+1·2	+1·2		32·5	100	Detached, the farther observations being trans-ferred to the Royal Military Academy.
6	60	+1·06	—0 3·9	....					
7	59	......	—0 3·1	+0·8					
8	59	......	—0 3·3	—0·2					
9	57	—0·7	—0 4·5	—1·2					
10	57	......	—0 4 5	—0·0	+0·2	—1·3	33	97	The Chronometer, during this interval, wa placed to the East of the ball, 2 inches from the floor, and distance from the vertica through the centre of the ball 11·1 inches corresponding to lat. 20° 45′, long. 7° 45′ central distance 12. 12 o'clock, South.
11	57	......	—0 4·0	+0·5					
12	60	......	—0 3·5	+0·5					
13	56	—0·25	—0 3·0	+0·5					
14	54	—0 0	—0 2 5	+0·5					
15	54	......	—0 0·9	+1·6	+1·5	....	32·5	100	Again detached from the ball.
16	54	......	+0 0·6	+1·5					
17	54	......	+0 2·3	+1·7					
18	55	—0·1	+0 3·3	+1·0					
19	56		+0 3·55	+0·25	+1·1	—0·4	41·0	63	Placed to the South of the ball, 1 inch from the floor, 10 inches from the vertical, or lat 9° 19′ N. long. 90° central dist. 11·4 inches. 12 o'clock, South.
20	55		+0 5·4	+1·75					
21	55	—0·25	+0 7·15	+1·75					
22	53		+0 7·95	+0·8	+1·3	—0·2	41·0	63	Same situation, but the 12 o'clock turned to the North.
23	50		+ 9·65	+1·7					
24	50	—0·2	+ 11·05	+1·4					
25	51		+ 12·35	+1·3					

* The clock rates from April 30th, are for the Astronomical Clock at the Royal Military Academy, by PENNINGTON.

TABLE V.

Experiments and observations on the rates of Chronometers in the vicinity of iron bodies, at the Observato of the Rev. Mr. EVANS, *Woolwich Common. Lat.* 51° 29′ 8″ *North, long.* 4′ 10″ *East.*

No. V. *Box Chronometer, by* PARKINSON *and* FRODSHAM. *Mean detached rate* +0·23.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten Compass Vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
	°		′ ″						
March 25	48	......	—0 36·1	....	+0·9	....	32·5	100	These rates were taken before the Chro meter was applied to the ball. This Ch nometer had not been wound up since tober 18, 1820; its rate was then —0·8.
26	47	......	—0 35·0	+1·1					
27	49	—2·4	—0 34·5	+0·5					
28	48	......	—0 34·2	+0·3					
29	50	......	....	+1·2					
30	48	—2·4	—0 31·7	+1·2					
31	49	......	—0 35·5	—3·8	—3·4	—3·6	27·2	143	Placed to the South of ball; height 9·8 inc from floor; distance from vertical 11·3 ches, or lat. 35° 16′ S. long. 90°, central tance 12 inches. 12 o'clock, South.
April 1	48	—2·5	—0 39·8	—4·2					
2	51	—1·7	—0 42·2	—2·4					
3	50	......	—0 45·3	—3·1					
4	49	—1·9	—0 47·9	—2·6	—3·3	—3·5	27·2	143	Same situation. 12 o'clock, North.
5	49	—2·5	—0 51·0	—3·1					
6	47	......	—0 55·1	—4·1					
7	50	......	—0 58·3	—3·2	—2·5	—2.7	27·2	143	Same situation. 12 o'clock, West. Not wo up on the 7th.
8	....	—1·9	—0 16·2	....					
9	57	—1·9	—0 17·9	—1·7					
10	58	—1·7	—0 16·4	+1·5	+0·7	+0·5	27·2	143	Same situation. 12 o'clock, East.
11	57	......	—0 14·8	+1·6					
12	55	—1·7	—0 15·9	—1·1					
13	53	—2·0	—0 18·3	—2·4	—3·9	—4·1	27·2	143	Same situation; but the 12 o'clock turned to the South, as on March 31st, April and 2nd.
14	52	......	—0 21·9	—3·6					
15	50	......	—0 25·5	—3·6					
16	49	......	—0 31·3	—5·8					
17	49	—1·9	—0 35·5	—4·2					
18	51	—2·1	—0 41·5	—6·0	—3·2	—3·4	*	......	Placed on pedestal to the South of the p height above the centre of the plate 10 ches; and distance from vertical thr centre of plate, 10 inches. 12 o'c South. See description of plate, page 3
19	53	......	—0 45·0	—3·5					
20	55	......	—0 47·6	—2·6					
21	57	......	—0 49·0	—1·4					
22	56	—2·4	—0 51·5	—2·5					
23	56	—2·4	—0 53·2	—1·7	+0·4	....	32·5	100	Detached both from ball and plate, to asce whether it would return to its forme tached rate.
24	60	—1·5	—0 52·8	+0·4					
25	63	......	—0 52·6	+0·4					
26	65	—0·8	—0 49·4	+3·2					
27	63	—0·3	—0 48·0	+1·4					
28	62	—0·5	—0 48·7	—0·7					
29	60	—0·9	—0 46·6	+2·1					
30	56	......	—0 48·3	—1·7					

* The intensity in this case was not taken, in consequence of the plate having been sent on board the Fury, Captain PARRY, f purpose of correcting the local attraction of that vessel, before it was recollected that this datum had not been obtained.

Observations on No. V. continued at the Royal Military Academy.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten Compass Vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
From Ap. 30 to May 6.	°	* 1·06	....	″ —0·6	—0·6	....		100	Detached. The farther observations transferred to the Royal Military Academy.
7	59	......	+0 50·4	—1·7	—1·4	—1·6	27·0	150	Placed to North of ball on the floor; height 1 inch; distance from vertical 12 inches, or lat. 44° 8′ N. long. 90°, central distance 13·2 inches. 12 o'clock, South.
8	59	......	+0 49·2	—1·2					
9	57	—0·7	+0 48 0	—1·2					
10	57	......	+0 46·5	—1·5	—2·1	—2.3	30·0	117	Placed to the North of ball; height from floor 6½ inches; distance from vertical 10 inches, corresponding to lat. 19½° N. long. 90°, central distance 10 inches. 12 o'clock, South.
11	57	......	+0 45·5	—1 0					
12	60	......	+0 42·5	—3·0					
13	56	—0 25	+0 40·5	—2·5					
14	54	—0·0	+0 37·5	—2·5					
15	54	......	+0 36·2	—1·3	—1·7	—1·9	23·0	199	Placed to the South of ball; height 8 inches, distance from vertical 10, or lat. 28°·2′ S. long. 90°. central dist. 10·1 inch.; 12 o'clock, S.
16	54	......	+0 34·3	—1·9					
17	54	—0.1	+0 32·5	—1·8					
18	55	......	+0 29 9	—2·6	—2·3	—2·5	22·5	208	Placed South of ball; height 12 inches; distance 10 inches, corresponding to lat. 48° 30 S. long. 90°, central distance 11·4 inches. 12 o'clock, South.
19	56	......	+0 26·7	—3·2					
20	55	......	+0 24·7	—2·0					
21	55	—0·25	+0 23·2	—1·5					
22	53	......	+0 19·6	—3·6	—1·7	—1.9	22·5	208	Same situation, but with the 12 o'clock turned to the East.
23	50	......	+0 18·4	—1·2					
24	50	—0·20	+0 17·2	—1·2					
25	51	......	+0 16·5	—0·7					

* The clock rates from April 30th, are for the Astronomical Clock at the Royal Military Academy, by Pennington.

TABLE VI.

xperiments and observations on the rates of Chronometers in the vicinity of iron bodies, at the Observatory of the Rev. Mr. Evans, *Woolwich Common. Lat.* 51° 29′ 8″ *North, long.* 4′ 10″ *East.*

No. VI. *Box Chronometer, by* Parkinson *and* Frodsham. *Mean detached rate* —0·39.

Days.	Thermometer.	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten Compass vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
	°		′ ″						
pril 9	57	—1·9	+3 16·7		+0·2		32·5	100	These rates were taken before the Chronometer was applied to the ball.
10	58	—1·7	+3 17·0	+0·3					
11	57	.....	+3 17·1	+0·1					
12	55	—1·7	+3 17·2	+0·1					
13	53	—2·0	+3 17·2	0·0	—1·3	—0·9	33	97	Placed to the East of ball; height from floor 2 inches; distance from vertical 11·1 inches, corresponding to lat. 20°45′ N. long. 7° 45′, distance from centre 12 inches. 12 o'clock, South.
14	52	......	+3 15·7	—1·5					
15	50	......	+3 14·2	—1·5					
16	49	......	+3 12·4	—1·8					
17	49	—1·9	+3 10·5	—1·9					
18	51	—2·1	+3 9·2	—1·3	—1·5	—1·1	33	97	Similar situation, and at the same distance to the West of the ball. 12 o'clock, South.
19	53	......	+3 7·1	—2·1					
20	55	......	+3 5·6	—1·5					
21	57	......	+3 4·2	—1·4					
22	56	—2·4	+3 2·9	—1·3					
23	56	—2·4	+3 0·5	—2·4	—1·6	—1·2	30·5	127	Placed to the North of the ball; height 2 inches, distance 14 inches; or lat. 37°20′ N. long. 90°, central distance 14·6 inches. 12 o'clock, South.
24	60	—1·5	+2 59·5	—1·0					
25	63	......	+2 58·2	—1·3					
26	65	—0·8	+2 57·0	—1·2	—0·6		32·5	100	Detached; and the farther observations transferred to the Royal Military Academy.
27	63	—0·3	+2 57·3	+0·3					
28	62	—0·5	+2 57·6	+0·3					
29	60	—0·9	+2 56·8	—0·8					
30	56	.....	+2 55·2	—1·6					

Observations on No. VI. *continued at the Royal Military Academy.*

Days.	Thermometer	Clock Rate.	Chronometer + or — at Noon.	Daily Rate of Chronometer.	Mean Rate in each position.	Gain or Loss per day in each position.	Time of ten Compass vibrations.	Proportional Magnetic intensity.	Position of Chronometer, Remarks, &c.
From Ap. 30 to May 6.	60°	* +1·06	+1′ 34″·6	—0·5	—0·5	....	32·5	100	Observations begun at the Royal Military Academy.
7	59	....	+1 31·9	—2·7	—1·9	—1·5	34	91	Placed to the South of the ball; height 1 inch, distance from vertical 12 inches, or lat. 5° 8′ N. long. 90°, central distance 13·2 inches. 12 o'cloek, South.
8	59	....	+1 30·2	—1·7					
9	57	—0·7	+1 29·0	—1·2					
10	57	....	+1 27·5	—1·5	—1·4	—1·0	24	183	Placed still to the South; height 6½ inches, distance from vertical 10 inches, or lat. 19°30′ S. long. 90°, central distance 10 inches. 12 o'clock, South.
11	57	....	+1 26·0	—1·5					
12	60	....	+1 25·0	—1·0					
13	56	—0·25	+1 23·5	—1·5					
14	54	0·0	....	....	—1·4	—1·0	25	169	Placed to the North of the ball; height 1 inch, distance 10 inches from vertical, or lat. 48° 18′ N. long. 90°, central distance 11·4 inches. 12 o'clock, South.
15	54	....	+1 21·2	....					
16	54	....	+1 19·6	—1·6					
17	54	—0·1	+1 18·5	—1·1					
18	55	—0·1	+1 17·9	—0·6	—1·2	—0·8	56	33	Placed still to the North; height 13 inches, distance from vertical 9 inches, or lat. 16°20′ S. long. 90°, central distance 11 inches. 12 o'clock, South.
19	56	....	+1 16·6	—1·3					
20	55	—0·25	+1 15·6	—1·0					
21	55	....	+1 13·6	—2·0					
22	53	....	+1 12.9	—0·7	—1·0	—0·6	56	33	Same situation, but with the 12 o'clock turned to the North.
23	50	....	+1 11.7	—1·2					
24	50	—0·20	+1 10.7	—1·0					
25	51	....	+1 9.5	—1·2					

* The clock rates from the 30th, are for the Astronomical Clock of the Royal Military Academy, by PENNINGTON. The above chronometer is corrected according to the new principle of Messrs. PARKINSON and FRODSHAM. The rate of this chronometer for 8 days, after its return, was —0″·39.

Practical deductions from the results of the preceding experiments.

The first general conclusion which may be drawn from the foregoing experiments, is, that the rate of a chronometer is undoubtedly altered by its proximity to iron bodies.

Secondly ; it appears that it is by no means a general case, that iron necessarily accelerates the rate of a chronometer, as would appear from Mr. Fisher's observations ; for five out of the six chronometers which I have made use of, were obviously retarded in every situation in which they were placed. In one instance only, viz. chronometer No. II, there is an indication of acceleration in one situation ; but it is more doubtful than the retardation in all the other five.

It is also very obvious from the experiments on Nos. IV. and V., that much depends on the direction of the balance with respect to the iron : thus, No. IV. lost nearly 2″ per day when its 12 o'clock hour mark was turned to the South, and only seven tenths when it was placed to the East; but as soon as the chronometer was returned to its old direction, the loss again became 2″·1 daily. The same occurred in the case of No. V., which lost 3″·6 per day in one direction, and gained 0″·5 in another at right angles to it; and on returning it again to its former direction, the losing rate became 4″·1 per day, viz. rather stronger than at first. It must be admitted, however, that the same striking difference in the rate, as depending upon direction, was not observed in another instance, when a similar experiment was repeated on the same chronometer. Speaking generally, it also appears, that the greatest effect is produced in those instances where the change in the magnetic intensity is the greatest ; but there does not seem

to be that uniformity of relation in these cases, that we should naturally have anticipated.

As a practical conclusion, it is obvious, that on ship-board, great care ought to be taken to keep the chronometers out of the immediate vicinity of any considerable mass, or surface of iron; on which account, they ought not to be kept in the cabins of the gun-room officers, which are on the sides of the vessel; and probably a strong iron knee, or even a gun, will be found at a very inconsiderable distance from the spot where the watch is most likely, in this case, to be deposited.

In short, it appears from the preceding experiments, that a chronometer ought to be kept as carefully at a distance from any partial mass of iron, as the compass itself. And, as much of the iron of a ship is hidden, the best way of detecting it, and of ascertaining a proper situation for a chronometer, will be to set down a compass in any place designed for the former, and to observe and compare the direction of its needle with that of the azimuth compass on deck, while the vessel is on different tacks; and if the disagreement between the two be very considerable, another situation ought to be selected.

When I made my experiments on local attraction, on board His Majesty's ship Leven, we placed several compasses in different parts of the vessel, some of which were very powerfully affected under different directions of the ship's head; in consequence, no doubt, of their being within the influence of partial action arising from some near, but hidden, mass of iron.

In support, and in confirmation of the necessity of taking the above precautions, it may not be amiss to state the fol-

lowing fact. A very intelligent seaman, many years a Master in the Navy, and at present an officer in the Dock-yard at Woolwich, to whom I was describing the nature of my experiments, immediately exclaimed, that they explained a circumstance which he had remarked when he was master of a first rate. He informed me, he always found that his chronometer, which was a very excellent one, had a different rate on board and on shore, amounting to 5″ per day; but as he well remembered that the birth he had selected for it was in his cabin, nearly in contact with an iron knee, he now saw that it was the action of that mass of iron which had caused all his perplexity.

Lastly; since it is rendered obvious by the experiments with the plate of iron on Nos. IV. and V. that the power of the iron to disturb the action of the chronometer resides (as in the instance of the compass), on the surface, and as we know, generally, the distance and direction of such a plate, so that its power may be equal to the mean action of the iron of the vessel, we have thence a ready method of ascertaining, before a chronometer is sent on board, whether the effect of the ship's iron will be to accelerate or retard its going; and probably, a very near approximation to the actual quantity of that change may also be predicted.

For this purpose, it is only necessary to have a box or pedestal, as shown in Figure 3, Plate XXV., in the side of which a brass pin, *a b*, may be fixed, to carry the iron plate P, and on the top of the box a convenience for placing the chronometer. Then, having taken its rate in the usual way, let it be taken again while the chronometer is placed on the pedestal, keeping the plate, generally, at the distance of about

twelve inches from the vertical through the centre of the dial, and its centre about the same depth below the plane of the balance, and the rate thus obtained will be a very close approximation to the ship rate of the instrument, provided care be taken, when it is removed on board, to keep it out of the immediate action of any partial mass of iron. The plate for this purpose should be a double one, such as I have described in my "Essay on Magnetic Attraction," and if it weigh about 5lbs. it will be sufficient to prevent any partial action.

It should be observed, that the plate is meant as a substitute for the iron forward; and therefore the chronometer, when on board, should be placed in the same direction in reference to the ship's head, as it had with respect to the iron plate when its rate was determined; that is, if the 12 o'clock mark of the dial be turned towards the iron plate on shore, then must the same be turned towards the ship's head when taken on board.

Experiments on the detached parts of a chronometer.

As some of the results of the preceding experiments were not precisely what I had anticipated, nor quite consistent with the ideas I had formed of the nature of the action between the iron and the balance, I was desirous of making some experiments on the detached chronometrical parts, in order, if possible, to trace the irregularity to its source.

Having mentioned my wish on this subject to Mr. Frodsham, he very cordially and earnestly entered into my views, desirous, not of avoiding, but of meeting openly every difficulty which presented itself in the construction of such a

machine, the most delicate, perhaps, of any in the entire circle of the mechanical arts.

We accordingly went into his work-shop, and having detached a balance from a chronometer, we suspended it very nicely in its frame, and brought it near a piece of iron of some magnitude, which happened to be at hand, and an action between it and the balance was rendered immediately obvious; and it was of that kind which seemed to imply, that it proceeded from the magnetism of the balance, or of the spring which remained attached to it; that is, if the motion which we gave to the balance terminated in a certain place, a trifling recoil, or repulsion, might be distinguished; but if the opposite side of the balance was nearest to the iron when the motion ceased, then, a slight degree of attraction was equally distinguishable; and Mr. Frodsham had no doubt that such an action as we then noticed, was amply sufficient to change the rate of the chronometer, of which the balance formed a part, when brought within the sphere of attraction of any such iron mass.

The above experiment was made with the balance and frame placed near the bottom of the piece of iron; it was now repeated near its upper part, and a similar action was distinguishable; but it appertained to the reverse extremities of the balance.

I have said, that these results were such as indicated the presence of magnetism in the balance or spring; and it may not be amiss to advert here to this subject a little more particularly, and to explain how I imagine we may always distinguish between the magnetism of the balance, and that of the attracting body.

1st. If the balance have a polar, or directive quality, and the iron is pure and free from it (except that which is due to position); then, if the balance be kept below the *plane of no attraction*, the south pole of the former will be attracted, and its north pole repelled; but if the balance be placed above the plane of no attraction, the reverse will take place; that is to say, that part of the balance which was before repelled will be attracted, and that which was attracted will be repelled; and the same will happen, whichever of the ends or parts of the iron be turned downwards.

Therefore, when such action as that above described takes place, we may infer that the balance is magnetic, but that the iron, or attracting mass, is free from any polar quality, except that which it derives from position.

2. If the iron and balance were both magnetic, then we should have attraction and repulsion, as above described; but it would have no reference to the plane of no attraction; and by inverting the position of the iron, the effects of it upon the balance would be reversed also.

3. Again; if the iron possess the polar quality, but the balance and spring are free from it, then in every situation, either side or part of the balance, which is nearest to one of the poles of the attracting body, will be attracted, and no repulsion will in such case be observed.

4. Lastly; I am of opinion (although it is here, as in most other cases, difficult to prove a negative) that no action whatever will take place between the balance and iron, provided they are both free from any fixed polar quality.

As we were not prepared to pursue our enquiries any farther at this time, Mr. Frodsham proposed to provide himself

with certain parts of a chronometer, and to appoint a day to come to Woolwich, and make such experiments as might suggest themselves to him or to me in the interval. He therefore prepared for the purpose a new compensation balance, in which, of course, the usual care was taken to prevent the excitement of any local magnetism; he also brought with him a brass balance, with two springs of different tempers, which might, either of them, be affixed to the balance in the usual way; he had likewise, beside the proper frame for suspending these parts, constructed a brass stand, whereby the whole might be nicely adjusted to horizontality.

Our first experiment was on the new compensation balance; but although it was brought almost in contact with the iron ball, and at that place where, by means of our experiments, page 370, the intensity was known to be the greatest, no action whatever could be discovered. We afterwards repeated the same experiments in several other places, but without producing the least apparent effect. The weights of the balance were now removed, in order to render it more light and sensible; but no species of action could be discerned.

We now detached the balance entirely from the ball, and presented to it the north end of a bar magnet; and then, giving the balance a very slight motion, it stopped after a short time, and arranged itself, so that the cross steel bar was directly in a line with the magnet; and immediately, upon being disturbed from its position, it returned to it again. The balance being now turned half round, so that the other end of the bar was directed towards the magnet, the same effect was produced. We now turned the magnet end for end, but found the attraction still the same between either

end of the magnet, and on each end of the steel bar of the balance; and in no case could there be obtained the slightest indication of repulsion; from which we may conclude, that the balance itself was free from any polar magnetic quality, and that every part of it was alike susceptible of the power of the magnet, although it was wholly insensible to the action of the iron ball. A chronometer, therefore, with such a balance, and with a spring equally free from magnetism, would, I conceive, preserve the same rate both on shore and on shipboard, although it might be as sensibly affected with a magnet as any of those experimented upon by Mr. Fisher.

We now took the brass balance, and having suspended it in its frame, applied to it the end of the bar magnet, in order to ascertain whether any magnetic quality could be discovered in the brass of which it was composed; but no action of that kind could be rendered sensible. One of the springs being now attached to it, it was applied to the ball, whereby a small, but sensible, effect was produced by the action of the iron; and by repeating the experiment in various ways, it was obviously of that kind which indicated magnetism in the spring; and a very similar action was discoverable with the other spring.

Each of these was now applied, at a short distance, to a very light and sensible compass needle; when the polar quality of both was rendered manifest in a very peculiar manner, but which it is not necessary to detail in reference to the present enquiry.

All we learn from these experiments appears to be, that when a balance, or its spring, acquires a magnetic quality, the rate of the chronometer, of which it forms a part, will

experience a change when brought within the action of a mass of iron; and, *a fortiori*, when it is approximated to a magnet; but, if the balance and its spring are both free from magnetism, then the chronometer will preserve its rate, notwithstanding the proximity of iron; but it will still be acted upon by a magnet.

I think it however highly probable, that the form and office of the spring, are precisely those the most likely to create magnetism in it, and that when once acquired in this part of the machine, it will be soon transmitted to the balance itself, and consequently, that there are but few chronometers, which have been long in use, that have not their balances impregnated with this subtle fluid, and which are therefore liable to a change of rate, more or less considerable, when taken on ship-board, or within the influence of a mass of iron.

I must acknowledge, however, that there is still some mystery hanging over this enquiry: the only reason that can be assigned for the effect produced by the iron in these cases is, that it has a tendency to increase or diminish the vibratory motion of the balance, which we must, I conceive, assimilate to the oscillations of a horizontal needle; from which it only appears to differ in its degree of directive intensity. But it will have been observed, that the nearest approach I could make to the iron, did not increase or diminish the intensity of this action so much, as in the ratio of 2 to 1, notwithstanding which a sensible effect was produced on the rate of the chronometers; whereas from Captain Parry's and Captain Sabine's observations at Melville island, it appears, that the directive power of the needle was reduced to $\frac{1}{13}$th of what it is in London, and yet no change, or a very inconsiderable one, was

Fig. 1.

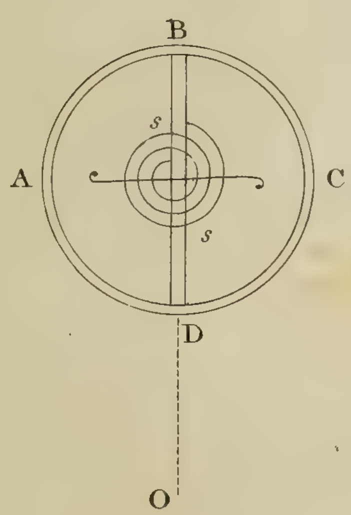


Fig. 2

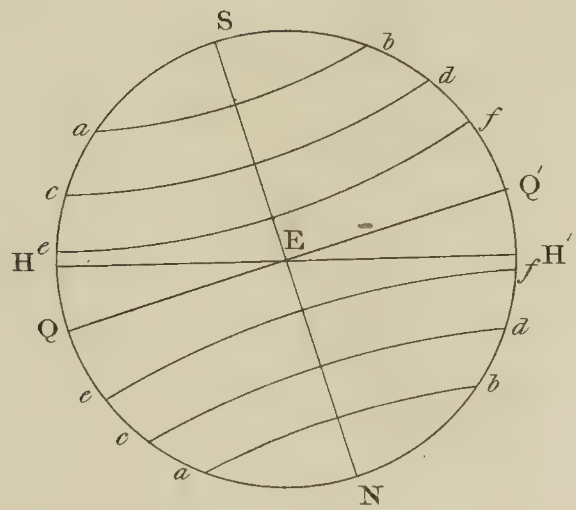
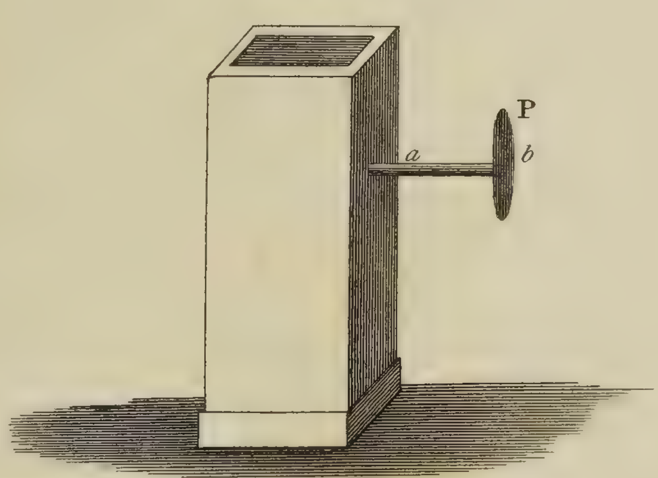


Fig. 3



J.s Basire sc.

observed in the rates of the chronometers ; and this change, from the results of the preceding table of experiments, would rather appear to be due to the action of the iron on board, than to any other cause.

On the other hand, the change of rate reported by Mr. Fisher, is so much greater than our experiments would give us reason to expect, that we cannot help considering his case as an anomalous one, and as depending upon some cause not commonly operating on ship-board.

XXVI. *On the peculiarities that distinguish the Manatee of the West Indies from the Dugong of the East Indian seas. By Sir* Everard Home, *Bart. V. P. R. S.*

Read July 12, 1821.

Having received from the Duke of Manchester, Governor of the Island of Jamaica, a manatee preserved in spirit, which is a species of dugong, but very different from that of which an account has so lately been read to the Society, I am desirous to add some observations upon this species, the whole tribe of animals having hitherto been little known with respect to their internal structure. The manatee differs in its external form from the dugong, the tail being much broader, and the ribs having greater lateral extension. As this animal feeds upon the plants that grow at the mouths of great rivers, and the dugong upon those met with in the shallows among small islands in the Eastern Seas, this difference of form will make it more buoyant and better fitted to float in fresh water; while its habits of life place it between the dugong and hippopotamus. There are no tusks. The snout is flattened, and upon the ends of its toes there are nails, as is shown in the annexed drawing, (Pl. XXVI.)

The teeth differ in number from those of the dugong, there being twenty-four molares, six on each side of each jaw.

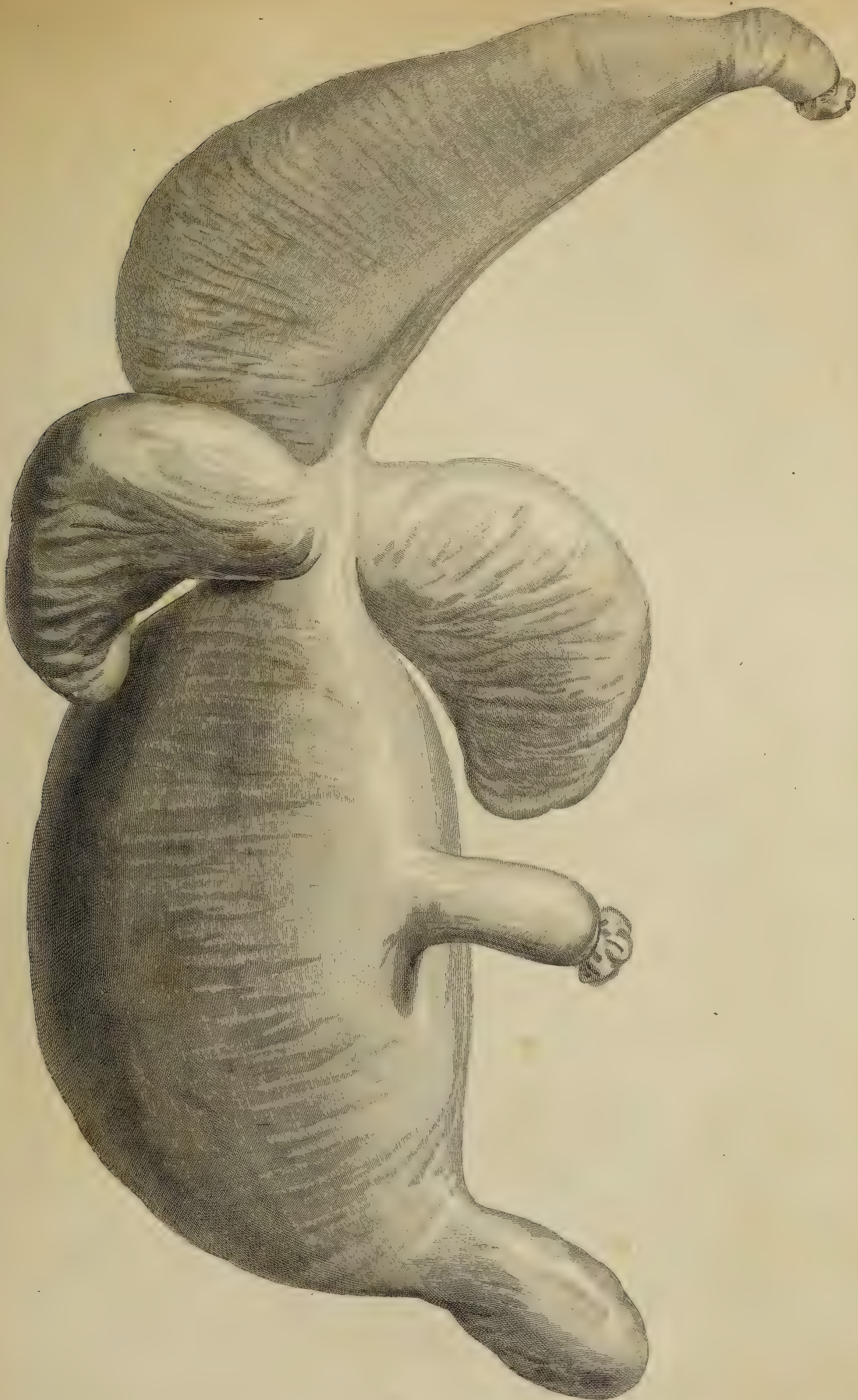
The skull has the orbit nearly a compleat circle; the intermaxillary bones are curved at their union, but the length

Scale 2 Inches to a Foot

Wm Clift del. *Js Basire sc.*

Scale 1 3/4 Inches to a Foot

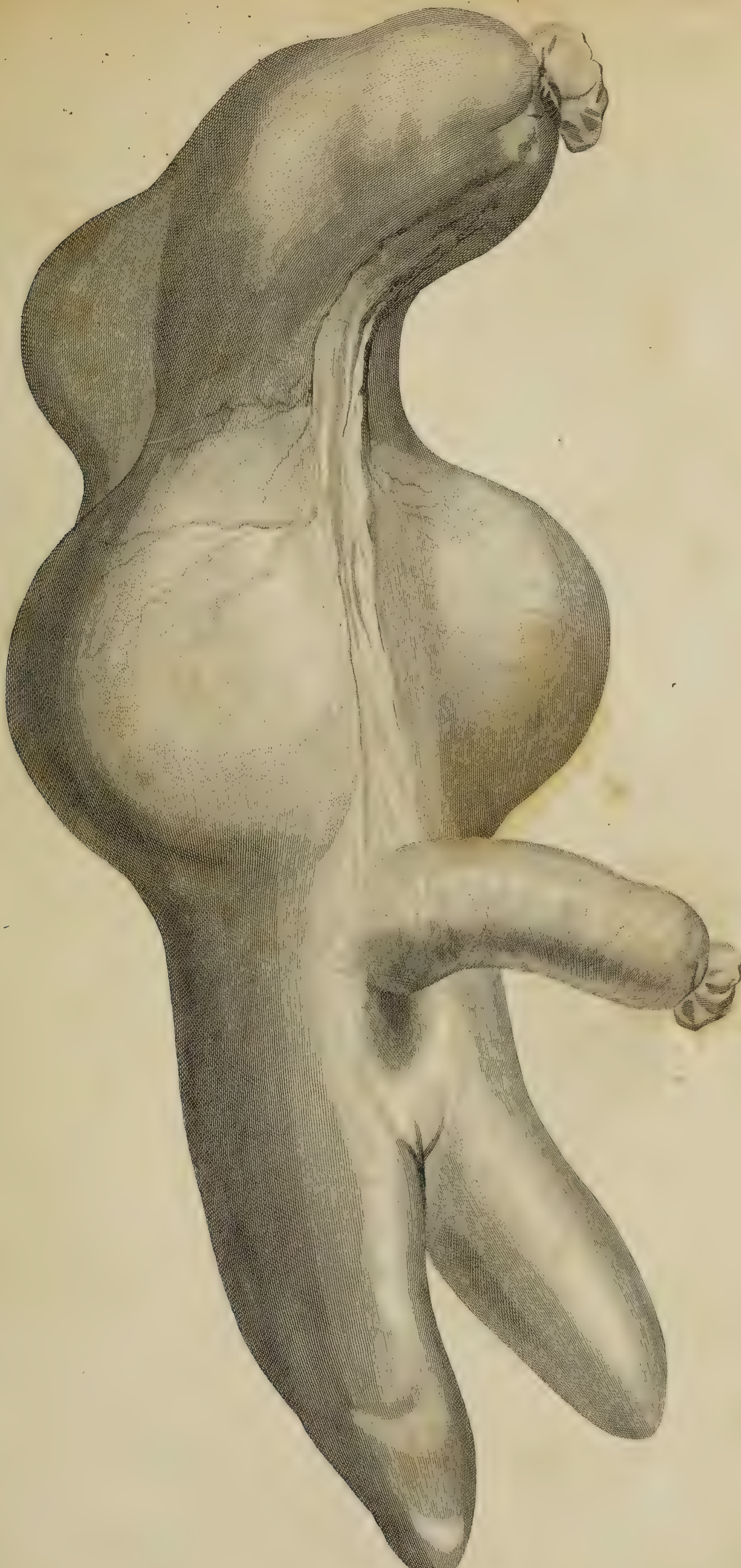
Wm Clift del 1821. *J. Basire sc.*



Scale half an Inch to an Inch

W. Clift del. 1821.

Ja. Basire sc.



Natural Size

Wm Clift del. — Jas Basire sc.

Charles Bell del. *J. Basire sc.*

of curvature is only two inches. The bones of the ear resemble those of the dugong.

The skeleton in the general view is similar to that of the dugong. The number of vertebræ is forty-eight, seven to the neck, seventeen to the back, twenty-four to the tail, which last have long transverse processes tipped with cartilages. The ribs are thicker and more massy than in the dugong, and considerably more spread out. There are seventeen on each side. The great and little toe have only two phalanges. The toe next the great one has three, the third four, and the fourth toe three phalanges: these are shown in the drawing of the skeleton.

The stomach differs from that of the dugong in the solid glandular part being more pyramidal, and connected to the general cavity by a neck, and the two lateral pouches being wider and shorter, the posterior the largest.

The food was found to be fuci.

The cæcum consists of a large globular bag with two finger-like hollow processes, unlike the cæcum in other animals, and therefore is represented in the annexed drawing.

The uterus resembles that of the dugong.

The heart and lungs were not in a state to be examined.

In these two species of this extraordinary tribe of animals, between which there is so great a resemblance, the teeth are totally different, which shows the mode of classing animals from the appearance of the teeth to be very erroneous.

The engravings, Plates XXVI. XXVII. XXVIII. XXIX. require no particular explanation, as the scale is marked on the plates.

XXVII. *On a new compound of Chlorine and Carbon. By* Richard Phillips, *F. R. S. E. F. L. S. M. G. S. &c. and* Michael Faraday, *Chemical Assistant in the Royal Institution. Communicated by Sir* Humphry Davy, *Bart. P. R. S.*

Read July 12, 1821.

M. Julin, of Abo, in Finland, is proprietor of a manufactory in which nitric acid is prepared, by distilling calcined sulphate of iron with crude nitre in iron retorts, and collecting the products in receivers connected by glass tubes, in the manner of Woulfe's apparatus. In this process he observed, that when a peculiar kind of calcined vitriol, obtained from the waters of the mine of Fahlun, and containing a small portion of pyrites, known in Sweden by the name of calcined aquafortis vitriol No. 3, was used, the first tube was lined with sulphur, and the second with fine white feathery crystals. These were in very small quantity, amounting only to a few grains from each distillation; but M. Julin, by degrees, collected a portion of it, and, having brought it to this country, inserted a short account of its properties in the Annals of Philosophy, Vol. i. p. 216, to which a few observations were added by ourselves.

The following are the properties of this substance, as described by M. Julin. It is white; consists of small soft adhesive fibres; sinks slowly in water; is insoluble in it whether

hot or cold; is tasteless; has a peculiar smell, somewhat resembling spermaceti; is not acted on by sulphuric, muriatic, or nitric acid, except that the latter by boiling on it gives traces of sulphuric acid; boiled with caustic potash, has a small portion of sulphur dissolved from it; dissolves in hot oil of turpentine, but most of it crystallizes in needles from the solution on cooling; dissolves in boiling alcohol of .816, but by far the greater part crystallizes on cooling; burns in the flame of a lamp with a greenish blue flame, giving a slight smell of chlorine gas; when heated, melting, boiling and subliming at a temperature between 350° and 400°, and subliming slowly without melting at a heat of about 250°, forming long needles. Potassium burned with a vivid flame in its vapour in an open tube, and carbon was deposited; a solution made of the residuum, and saturated with nitric acid, gave a copious precipitate with nitrate of silver. M. JULIN then remarks, that the small quantity he possessed, with want of leisure, prevented him from making any farther experiments on it; and concludes, by comparing it with the chlorides of carbon that have lately been formed.

The small quantity of the substance which, by the kindness of M. JULIN, we had at our disposal at that time, was insufficient to enable us satisfactorily to ascertain its nature. We found it mixed with free sulphur, and sulphate and muriate of ammonia. When purified, our first object, in consequence of M. JULIN's suggestion, was to compare it with the per-chloride of carbon, but it was found entirely distinct from it in its properties.

Since M. JULIN's return from the continent, he has very kindly placed some farther portions of this substance at our

disposal. We have therefore been enabled to continue our experiments, and have come to the very unexpected conclusion of its being another chloride of carbon, in addition to the two, an account of which has been published in the Transactions of the Royal Society for this year.

The substance, after being boiled in solution of potash, washed in water, dried and sublimed, formed beautiful acicular crystals, which appeared to Mr. W. PHILLIPS to be four-sided prisms. They contained no sulphur, and, when dissolved in alcohol or ether, gave no traces of chlorine or muriates, by nitrate of silver. They burned in the air with a strong bright flame at a heat below redness, and agreed with the description given by M. JULIN of the properties of the substance.

When heated moderately, it sublimed unaltered; but on passing a portion over rock crystal, heated to bright redness, in a green glass tube, it was decomposed, charcoal was deposited, and the gas, passed into solution of nitrate of silver, precipitated it, and proved to be chlorine.

A portion was repeatedly sublimed in a small retort filled with chlorine, which was made red hot in several places; it however underwent no change; but on cooling crystallized as at first. It was also exposed in the same gas to sun light for many days, but no change took place.

When raised in vapour over hot mercury, and detonated with excess of oxygen, a quantity of carbonic acid gas and chloride of mercury were produced. There was no change in the volume of gas used; and lime water being passed into it, absorbed the carbonic gas, became turbid, and left a residuum of pure oxygen. Acetic acid being then added, to dissolve the carbonate of lime, the solution was tested for

chlorine, which was readily found in it. When detonated with oxygen, the substance being in excess, there was expansion of volume, carbonic oxide, carbonic acid, and chloride of mercury being formed.

When phosphorus, iron, tin, &c. were heated to redness in its vapour over mercury, it was decomposed, chlorides of those substances being formed, and charcoal deposited; and M. JULIN has shown that the same effect is produced by potassium.

Three grains of this substance were passed in vapour over pure peroxide of copper, heated to redness in a green glass tube: a very small portion passed undecomposed. The gas received over mercury equalled 5.7 cubic inches; it was carbonic acid gas. A small part of the oxide of copper was reduced, and portions of a crystalline body appeared within the tube, which, on examination, proved to be chloride of copper. Some of this was used in making experiments on its nature; but when that was ascertained, the remaining contents of the tube were dissolved in nitric acid, and precipitated by nitrate of silver: 6.1 grains of chloride of silver were obtained.

Two grains were passed over pure quick lime, raised to a red heat in a green glass tube. The moment the vapour came in contact with the hot lime, ignition took place, and the earth burned as long as the vapour passed over it. When cold, the tube was examined, and much charcoal found deposited at the spot where the ignition occurred. The contents of the tube were dissolved in nitric acid, and the filtered solution precipitated by nitrate of silver: 5.9 grains of chloride of silver were obtained.

These results afford us sufficient data from which to deduce the nature and composition of this body. All the experiments of decomposition indicate it to contain chlorine and carbon, and those with oxygen and the metals, sufficiently prove the absence of hydrogen and oxygen. With regard to the proportions of the elements, three grains of the substance gave 5.7 cubic inches of carbonic acid gas, therefore two grains will give 3.8 cubic inches. One hundred cubic inches of carbonic acid gas weigh 46.47 grains, and contain 12.72 grains of carbon; and 3.8 cubic inches will therefore contain 0.483 grains of carbon. The two grains of the substance decomposed by heated lime gave 5.9 grains of chloride of silver, which, according to Dr. WOLLASTON's scale, equal 1.45 of chlorine; hence the two grains gave chlorine - - - 1.45

	carbon - - -	.483
		1.933

The loss here is 0.067, which is by no means important, when the small quantity of the substance and the nature of the experiments are considered.

As to the proportion of these two bodies to each other, if we consider chlorine as represented by 33.5, and carbon by 5.7, or with Dr. WOLLASTON by 44.1 and 7.5, then the 1.45 of chlorine would be equivalent to 0.2466 of carbon. This is the constitution of the fluid or proto-chloride of carbon; and if we double the 0.2466, the product 0.4932, approaches so near to the experimental result 0.483, that we do not hesitate to regard this compound as consisting of one portion of chlorine and two portions of carbon, or

chlorine - - -	44.1 - - -	33.5
carbon - - -	15 - - -	11.4

It is remarkable, that another of these compounds should be found so soon after the discovery of the two former chlorides of carbon. Its physical properties, and its chemical energies, are in every respect analogous to those of the former compounds; and its constitution increases the probability, that another chloride of carbon may be found, consisting of two portions of chlorine and one of carbon.

All the endeavours we have yet made to form the chloride of carbon now described, or to convert it into either of the other chlorides, have been unsuccessful. We expected that when decomposed by heat, it would produce the protochloride with the liberation of carbon, as the perchloride does with the liberation of chlorine, but we have not yet been able to ascertain that point. We have only to offer as an apology for this and other imperfections in the present paper, the smallness of the quantity of this substance that we possessed.

XXVIII. *On the Nerves; giving an account of some experiments on their structure and functions, which lead to a new arrangement of the system.* By Charles Bell, *Esq. Communicated by Sir* Humphry Davy, *Bart. P. R. S.*

Read July 12, 1821.

During the general advancement of science which has lately taken place in this country, observations have been gradually accumulating in the schools of the metropolis, which prove that the department of Anatomy has not been stationary. The nervous system, hitherto the most unsatisfactory part of a physiologist's studies, has assumed a new character. The intricacies of that system have been unravelled, and the peculiar structure and functions of the individual nerves ascertained; so that the absolute confusion in which this department was involved has disappeared, and the natural and simple order has been discovered.

In proceeding to give some account of these new observations, the Author of this paper had conceived, that it would be more suitable to the scientific body he had to address, to lay the subject before them in the precise manner in which it first presented itself to his enquiries, and to detail his observations and experiments in the succession in which they were made; but he has been persuaded by some of the Members of this Society to change that form, and to present the subject in the manner to which he has been

accustomed, in teaching these doctrines; and they were pleased to say, that in this way, a new subject would be more readily comprehended.

Intricacy of the nervous system.

Anatomists have of late, not only in this country, but also in Germany and Italy, made great improvement in the minute dissection and display of the nerves; but whilst the doctrines hitherto received prevail, the discovery of new branches of nerves, and new ganglia, only involve the subject in deeper obscurity. Whilst the nerves are supposed to proceed from one great centre, to have the same structure and functions, and to be all sensible, and all of them to convey what has been vaguely called nervous power, these discoveries of new nerves and ganglia are worse than useless; they increase the intricacy, and repel enquiry. The endless confusion of the subject induces the physician, instead of taking the nervous system as the secure ground of his practice, to dismiss it from his course of study, as a subject presenting too great irregularity for legitimate investigation or reliance.

When the physiologist sees two distinct nerves spreading their branches to every part of the face, (as in Pl. XXX.) three nerves from different sources given to the tongue, four to the throat, and nerves in most perplexing intricacy to the neck; when he finds one nerve with numerous ganglia or knots upon it, and another without them; when, in short, after a minute dissection of the nervous system, he finds a mesh, or network, spreading everywhere, it is not surprising that the seeming intricacy and confusion should make him, in despair, resign enquiry. But the Author of this paper being forced, in the

course of his duty, to go minutely over the demonstration of the nerves, year after year, without allowing himself to resign the subject merely on account of its intricacy, and finding the facts which he had to explain in his demonstrations of the anatomy, quite inconsistent with the received opinions, he gradually, after much study, was enabled to decypher and to read that language, of which the character had hitherto been imperfectly known; and now even the youngest students are brought to comprehend so much of the subject, that the idea of chance, or accident, or real confusion among these numerous branches, is entirely dismissed; and what remains unexplained has, by the success of our past enquiries, become a subject of peculiar interest, from the conviction, that attention to the minute anatomy, under the guidance of cautious and fair induction, will sooner or later lead to a comprehension of the whole system.

Statement of the object of the paper.

The Author means to limit his present enquiry to *the nerves of respiration.* But according to his conception of this matter, these nerves form a system of great extent, comprehending *all the nerves which serve to combine the muscles employed in the act of breathing and speaking.*

The first point of enquiry naturally is, how many of the muscles are combined in the act of respiration? and the second question, by what means are these muscles, which are seated apart from each other, and many of them capable of performing distinct offices, combined together in respiration? It may sound oddly to speak of the respiratory nerve of the face, of the neck, and of the shoulder; and it may be neces-

sary to give an illustration of the sense in which the term is intended to be employed. When a post-horse has run its stage, and the circulation is hurried and the respiration excited, what is his condition? Does he breathe with his ribs only; with the muscles which raise and depress the chest? No. The flanks are in violent action; the neck as well as the chest is in powerful excitement; the nostrils as well as the throat keep time with the motion of the chest. So if a man be excited by exercise or passion, or by whatever accelerates the pulse, the respiratory action is extended and encreased; and, instead of the gentle and scarcely perceptible motion of the chest, as in common breathing, the shoulders are raised at each inspiration, the muscles of the throat and neck are violently drawn, and the lips and nostrils move in time with the general action; and if he does not breathe through the mouth, the nostrils expand, and fall in time with the rising and falling of the chest; and that apparatus of cartilages and muscles of the nose (which are as curious as the mechanism of the chest, and which are for expanding these air tubes) are as regularly in action as the levator and depressor muscles of the ribs.

It is quite obvious, that some hundred muscles thus employed in the act of breathing, or in the common actions of coughing, sneezing, speaking, and singing, cannot be associated without cords of connection or affinity, which combine them in the performance of these actions: the nerves which serve this purpose I call respiratory nerves.

The nerves of the animal frame are complex, in proportion to the variety of functions which the parts have to maintain.

When we minutely and carefully examine the nerves of the human body, and compare them with those of other animals, a very singular coincidence is observed between the number of organs, the compound nature of their functions, and the number of nerves which are transmitted to them. No organ which possesses only one property, or endowment, has more than one nerve, however exquisite the sense or action may be; but if two nerves, coming from different sources, are directed to one part, this is the sign of a double function performed by it. If a part, or organ, have many distinct nerves, we may be certain that, instead of having a mere accumulation of nervous power, it possesses distinct powers, or enters into different combinations, in proportion to the number of its nerves. The knowledge of this circumstance gives new interest to the investigation of this part of anatomy.

Thus, in reviewing the comparative anatomy of the nerves of the mouth, we shall find, that in creatures which do not breathe, the mouth having only one function to perform, one nerve is sufficient. In certain animals where the face and nostrils have no complexity of relations, these parts have only a single nerve. If the throat has no complexity of organization, it has no variety of nerves. But on the other hand, when the anatomist employs weeks to dissect and disentangle the nerves of the tongue, throat, and palate, in the human subject, he finds at length, that he has exhibited the branches of five different trunks of nerves; and there is no clue to the labyrinth, until he considers the multiplied offices of the mouth

in man; that it is a pneumatic as much as a manducatory organ; that it is the organ of the voice and of speech, as of taste and exquisite feeling. It would indeed be matter of surprise, if the same nerve served for the action of gnawing and feeding in the lower animals of simple structure, and also for the governance of those complicated operations, which serve to interpret the wants and sentiments of man.

Such are the views which naturally arise from an acquaintance with the nerves of the human body; but a comparison of them, with those of the lower classes of animals, enables us to establish a more lucid order, and that not in an arbitrary manner, but perfectly according to nature.

The nerves of all creatures may be divided into two parts, or systems; the one simple and uniform, the other irregular and complex, in proportion to the complexity of organization.

When the nerves of the face, mouth, throat, and neck of the human subject are minutely displayed, it seems impracticable to reduce the numerous nerves which cross and entwine with each other to two distinct classes; yet nothing is more certain than that this may be done, and by an easy and natural method.

The principle which is to guide us, is obtained by ascertaining what parts of the organization of an animal are necessary to life and motion; what organs are superadded as the animal advances in the scale of existence, as necessary to higher and more complex enjoyments and actions.

Where an animal is endowed with mere sensation and locomotion, where there is no central organ of circulation, and no organ of respiration but what is generally diffused over the

frame, the nerves are extremely simple; they consist of two cords running in the length of the body, with branches going off laterally to the several divisions of the frame. And here no intricacy is to be seen, no double supply of nerves is to be observed, but each portion of the frame has an equal supply; and the central line of connection is sufficient to combine the actions of the muscles, and to give them the concatenation necessary to locomotion.

There is the same uniform and symmetrical system of nerves in the human body as in the leech or worm; although obscured by a variety of superadded nerves. These additional nerves belong to organs, which, tracing the orders of animals upward, are observed gradually to accumulate until we arrive at the complication of the human frame. These nerves, additional and superadded to the original system, do not destroy, but only obscure that system; and accordingly, when we separate certain nerves, the original system of simple constitution is presented even in the human body.

The nerves of the spine, the tenth or sub-occipital nerve, and the fifth or trigeminus of the system of Willis, constitute this original and symmetrical system. All these nerves agree in these essential circumstances; they have all double origins; they have all ganglia on one of their roots; they go out laterally to certain divisions of the body; they do not interfere to unite the divisions of the frame; they are all muscular nerves, ordering the voluntary motions of the frame; they are all exquisitely sensible; and the source of the common sensibility of the surfaces of the body: when accurately represented on paper, they are seen to pervade every part; no part is without them; and yet they are symmetrical and simple as the nerves of the lower animals.

If the nerves be exposed in a living animal, those of this class exhibit the highest degree of sensibility; while, on the contrary, nerves not of this original class or system, are comparatively so little sensible, as to be immediately distinguished; in so much that the quiescence of the animal suggests a doubt whether they be sensible in any degree whatever. If the *fifth nerve, and the portio dura of the seventh,* be both exposed on the face of a living animal, there will not remain the slightest doubt in the mind of the experimenter which of these nerves bestows sensibility. If the nerve of this original class be divided, the skin and common substance is deprived of sensibility; but if a nerve not of this class be divided, it in no measure deprives the parts of their sensibility to external impression.

More particularly of the respiratory nerves.

The nerves which connect the internal organs of respiration with the sensibilities of remote parts, and with the respiratory muscles, are distinguished from those of which we have been speaking by many circumstances. They do not arise by double roots; they have no ganglia on their origins; they come off from the *medulla oblongata* and the upper part of the spinal marrow; and from this origin, they diverge to those several remote parts of the frame which are combined in the motion of respiration. These are the nerves which give the appearance of confusion to the dissection, because they cross the others, and go to parts already plentifully supplied from the other system.

The following are the nerves to be enumerated as *respiratory nerves,* according to their functions.

1. *Par vagum*, the eighth of WILLIS, the *pneumogastric nerve* of the modern French physiologists. This nerve goes off from the common origin of the respiratory nerves, the lateral part of the *medulla oblongata;* it takes its course to the larynx, the lungs, the heart and stomach. It associates these organs together, which are at the same time supplied with nerves from other sources. Comparative anatomy would lead us to infer that this nerve is not essential to the stomach, as it does not exist but where there are heart and lungs to associate with a muscular apparatus of respiration. That the stomach must be associated with the muscular apparatus of respiration, as well as the lungs, is obvious, from the consideration of what takes place in vomiting and hiccough, which are actions of the respiratory muscles excited by irritation of the stomach.

2. *Respiratory nerve of the face*, being that which is called *portio dura* of the seventh. This nerve, like the last, goes off from the lateral part of the *medulla oblongata*, and, escaping through the temporal bone, spreads wide to the face. All those motions of the nostril, lips, or face generally, which accord with the motions of the chest in respiration, depend solely on this nerve. By the division of this nerve the face is deprived of its consent with the lungs, and all expression of emotion. This part of the enquiry will be found very interesting.

3. *Superior respiratory nerve of the trunk;* being that which is called *spinal accessory*. This nerve has exceedingly puzzled anatomists, from the singular course which it pursues. It arises from the superior part of the spinal marrow, in a line with the roots of the other respiratory nerves. Instead of

going directly out betwixt the vertebræ, as the regular spinal nerves do, it passes up into the skull, comes out through the skull with the par vagum, and, descending upon the neck, goes to the muscles of the shoulder. In this course it supplies muscles which are already profusely supplied by the regular system of nerves.

This nerve controuls the operations of the muscles of the neck and shoulder in their office as respiratory muscles, when, by lifting the shoulders, they take the load from the chest, and give freedom to the expansion of the thorax. When it is cut across in experiments, the muscles of the shoulder, which were in action as respiratory muscles, cease their co-operation, but remain capable of voluntary actions.

4. *Great internal respiratory nerve.* The *phrenic*, or *diaphragmatic*, of authors. This is the only nerve of the system which has been known as a respiratory nerve. Its origin, course, and destination, are so familiar to every one, that I shall not say anything more of it here. But there is another nerve, which has a remarkable resemblance to it, and which, from circumstances already noticed, has been entirely overlooked. This is

5. *The external respiratory nerve.* This has a similar origin with the preceding nerve. It comes out from the cervical vertebræ, and is connected with the phrenic nerve. It runs down the neck, crosses the cervical and axillary nerves, passes through the axilla, and arrives on the outside of the ribs, where, it is scarcely necessary to observe, the muscles are already supplied by nerves coming out betwixt the ribs from the system of regular nerves.

These four last mentioned nerves govern the muscles of

the face, neck, shoulders, and chest, in the actions of excited respiration, and are absolutely necessary to speech and expression. But there are other nerves of the same class which go to the tongue, throat, and windpipe, no less essential to complete the act of respiration. These are the glossopharyngeal nerve, the lingual, or ninth of Willis, and the branches of the par vagum to the superior and inferior larynx.

We proceed to examine these nerves in detail, and first

*Of the nerves of the face, in which it is shown that the two sets of nerves, hitherto supposed to be similar, differ in structure, sensibility, and function.**

It is in the face, that we have the best opportunity of observing the subservience of the nerves to the uses of the parts, and of ascertaining the truth of the preceding doctrines. The human countenance performs many functions: in it we have combined the organs of mastication, of breathing, of natural voice and speech, and of expression. These motions are performed directly by the will; here also are seen signs of emotions, over which we have but a very limited or imperfect controul; the face serves for the lowest animal enjoyment, and partakes of the highest and most refined emotions. Happily for our present object, the nerves, which in other parts of the frame are bound together for the convenience of distribution to remote parts, are here distinct, and run apart from each other until they meet at their extremities. They take different courses through the bones of the head, and come out upon the face, to be exposed in a manner which courts enquiry.

* This subject is illustrated by Plate XXX. which represents the nerves of the face.

The nerves of the face are, first, the *trigeminus*, or the *5th of* WILLIS, and that familiarly called the *portio dura* of the seventh, but which, in this paper, will be called *the respiratory nerve of the face*.

Of the trigeminus, or fifth pair.

In all animals that have a stomach, with palpi or tentacula to embrace their food, the rudiments of this nerve may be perceived; and always in the *vermes*, that part of their nervous system is most easily discerned which surrounds the œsophagus near the mouth. If a feeler of any kind project from the head of an animal, be it the antenna of the lobster or the trunk of an elephant, it is a branch of this nerve, which supplies sensibility to the member, and animates its muscles. But this is only if it be a simple organ of feeling, and is not in its office connected with respiration.

From the nerve that comes off from the anterior ganglion of the leech, and which supplies its mouth, we may trace up through the gradations of animals a nerve of taste and manducation, until we arrive at the complete distribution of the fifth, or trigeminus in man (see Plate XXX. B. C. D. which are its three grand divisions to the face.) Here in the highest link, as in the lowest, the nerve is subservient to the same functions. It is the nerve of taste, and of the salivary glands; of the muscles of the face and jaws, and of common sensibility. This nerve comes off from the base of the brain in so peculiar a situation, that it alone, of all the nerves of the head, receives roots both from the medullary process of the cerebrum and of the cerebellum. A ganglion is formed upon it near its origin, though some of its filaments pass on without entering into the ganglion. Before passing out of the skull

the nerve splits into three great divisions, which are sent to the face, jaws, and tongue. Its branches go minutely into the skin and enter into all the muscles, and they are especially profuse to the muscles which move the lips upon the teeth.

*Of the respiratory nerve of the face, being that which is called portio dura of the seventh.** (Plate XXX. A *a b c d*).

This nerve does not exist except where there is some consent of motions established betwixt the face and the respiratory organs. In fishes, this nerve, instead of being distributed forward to the face, passes backward to the muscles of the gills. In fact, there is, properly, no *portio dura* of the seventh in fishes, the nerve resembling it being a branch of the *par vagum.* A short description of this nerve in the human body will be necessary to our enquiry.

The respiratory nerve of the face arises from the superior and lateral part of the *medulla oblongata*, close to the *nodus cerebri*, and exactly where the *crus cerebelli* joins the *medulla oblongata.* The other respiratory nerves, which form so distinguished a part of the nervous system, arise in a line with the roots of this.

The nerve, passing into the internal auditory foramen, is here embraced by the *portio mollis;* but it separates from it, and is received into an appropriate canal of the temporal bone. A little farther on, and while within the temporal bone, two cords of communication are formed with the branches of the fifth nerve, or *trigeminus.* One of these is called Vidian nerve, and the other *corda tympani.* By these communications,

* *Portio dura nervi acustici. Sympatheticus parvus* by Winslow, *Faciale* by *Vicq. d'Azyr.*

nerves go in both directions; branches of the seventh are sent to the membrane of the nose, and to the muscles at the back of the palate; while branches of the fifth nerve (and also of the sympathetic nerve) are brought into the interior of the ear.

By the second of these communications, the *corda tympani*, [which joins the lingual branch of the fifth, just where that nerve is passing by the side of the *levator* and *circumflexus palati*,] the branches of this respiratory nerve have access to the *velum palati* and its muscles.

The respiratory nerve of the face, emerging through the stylomastoid foramen, divides into many branches, and these diverging, spread to all the side of the face. First, a branch is sent to the muscles of the outward ear; another is sent, under the angle of the jaw, to the muscles of the throat. The principal nerve then passes through the parotid gland and comes upon the face. Here the branches continue to scatter, to go upwards upon the temple, and downwards upon the side of the neck, forming on the neck a superficial plexus. The principal branches, however, go forward to the muscles of the forehead and eyelids; a branch called superior facial is sent to the muscles of the cheek and the side of the nose; while an inferior facial branch is given to the angle of the mouth and the muscles which concentrate there.

In this extensive distribution, the nerve penetrates to all the muscles of the face: muscles, supplied also with the branches of the fifth pair. Its branches penetrate to the skin, accompanying the minute vessels of the cheek.

The descending or inferior divisions, which go under the lower jaw and to the superficial muscles of the throat and

neck, are connected with branches of the spinal nerves, and with the respiratory nerves, as may be seen in the adjoined plate.

Its structure.

When we minutely observe the texture of the respiratory nerve of the face, we find it to correspond with the structure of the *par vagum*, and to differ from that of the *trigeminus.* The filaments of this nerve have a very close texture, like a minute plexus. The fifth, compared to it, has large free round filaments, with less intricacy in their texture.

If we were barely to consider this distribution of the *portio dura* of the seventh, unbiassed by theory or opinion, we should be forced to conclude, that it is not alone sufficient to supply any one part with nervous power, for every one of its branches is joined by divisions of the fifth. The question then naturally arises, whether these nerves perform the same function? whether they furnish a double supply of the same property or endowment, or whether they do not perform different offices? having taken all the assistance that the knowledge of the human structure and comparative anatomy afford, we are prepared to decide the matter by experiment.

Experiments on the nerves of the face.

An ass being thrown, and its nostrils confined for a few seconds, so as to make it pant and forcibly dilate the nostrils at each inspiration, the portio dura was divided on one side of the head; the motion of the nostril of the same side instantly ceased, while the other nostril continued to expand and contract in unison with the motions of the chest.

On the division of the nerve, the animal gave no sign of

pain; there was no struggle nor effort made when it was cut across.

The animal being untied and corn and hay given to him, he eat without the slightest impediment.

An ass being tied and thrown, the superior maxillary branch of the fifth nerve was exposed Touching this nerve gave acute pain. It was divided, but no change took place in the motion of the nostril; the cartilages continued to expand regularly in time with the other parts which combine in the act of respiration; but the side of the lip was observed to hang low, and it was dragged to the other side. The same branch of the fifth was divided on the opposite side, and the animal let loose. He could no longer pick up his corn; the power of elevating and projecting the lip, as in gathering food, was lost. To open the lips the animal pressed the mouth against the ground, and at length licked the oats from the ground with his tongue. The loss of motion of the lips in eating was so obvious, that it was thought a useless cruelty to cut the other branches of the fifth.

This experiment of cutting the respiratory nerve of the face, or *portio dura*, gave so little pain, that it was several times repeated on the ass and dog, and uniformly with the same effect. The side of the face remained at rest and placid, during the highest excitement of the other parts of the respiratory organs.

When the ass, on which the respiratory nerve of the face had been cut, was killed, which was done by bleeding, an unexpected opportunity was offered of ascertaining its influence, by the negation of its powers on the side of the face where it was cut across.

When an animal becomes insensible from loss of blood, the impression at the heart extends its influence in violent convulsions over all the muscles of respiration; not only is the air drawn into the chest with sudden and powerful effort, but at the same instant the muscles of the mouth, nostrils, and eyelids, and all the side of the face, are in a violent state of spasm. In the ass, where the respiratory nerve of the face had been cut, the most remarkable contrast was exhibited in the two sides of its face; for whilst the one side was in universal and powerful contraction, the other, where the nerve was divided, remained quite placid

From these facts we are entitled to conclude, that the *portio dura* of the seventh, is the respiratory nerve of the face; that the motions of the lips, the nostrils, and the velum palati are governed by its influence, when the muscles of these parts are in associated action with the other organs of respiration. These passages to the lungs are membranous tubes, moved by muscles, which serve to expand and widen them, so that the air may freely enter into the lungs. It is obvious that to produce this, these muscles must have a consent with the other muscles of respiration, and move simultaneously with them; and this is effected through the respiratory nerve of the face. It shall be proved in the sequel, that the throat, neck, shoulders, and chest, have similar nerves to this, similar in structure and function, and that these unite all the extended apparatus of breathing and speaking.

The actions of sneezing and coughing are entirely confined to the influence of the respiratory nerves. When carbonate of ammonia was put to the nostrils of the ass whose respiratory nerve had been cut, that side of the nose and face

where the nerves were entire, was curled up with the peculiar expression of sneezing; but on the other side, where the nerve was divided, the face remained quite relaxed, although the branches of the fifth pair and the sympathetic were entire. The respiratory nerve of one side of the face of a dog being cut, the same effect was produced; the action of sneezing was entirely confined to one side of the face.

These last experiments show, that the peculiar expression in sneezing, results from an impression on the respiratory nerves, and that the muscles of the face are drawn into sympathy solely by the influence of the respiratory nerve of the face.

There is no part of the nervous system where the anatomy has been more negligently consulted in forming our physiological opinions, than in what regards the office of the sympathetic nerve. The connections of this nerve, or rather system of nerves, being universal, it has been supposed that it was the chord through which the relations of the eye, nose, face, throat, diaphragm, &c. were established; whereas the combination is effected solely through those nerves which, from their grand or leading function, I have called the respiratory nerves.

It has been presumed, that the act of smiling is peculiar to the human countenance, and that in no other creature can there arise that state of enjoyment which produces this distinguishing character of the human face, the expression of benevolence, or of the enjoyment of the ridiculous. But every one must have observed how near the approach is to this expression, in a dog when he fawns on his master, and leaps and twists his body and wags his tail, while, at the same time,

he turns out the edge of the lips as like a laugh as his organs can express. When the respiratory nerve on one side of the dog's head was cut across, this motion of the lips no longer took place, although it was still observable on the other side, where the nerve was entire.

On cutting the respiratory nerve on one side of the face of a monkey, the very peculiar activity of his features on that side ceased altogether. The timid motions of his eyelids and eyebrows were lost, and he could not wink on that side; and his lips were drawn to the other side, like a paralytic drunkard, whenever he showed his teeth in rage.

We have proofs equal to experiments, that in the human face the actions of the muscles which produce smiling and laughing, are a consequence of the influence of this respiratory nerve. A man had the trunk of the respiratory nerve of the face injured by a suppuration, which took place anterior to the ear, and through which the nerve passed in its course to the face. It was observed that, in smiling and laughing, his mouth was drawn in a very remarkable manner to the opposite side. The attempt to whistle was attended with a ludicrous distortion of the lips; when he took snuff and sneezed, the side where the suppuration had affected the nerve remained placid, while the opposite side exhibited the usual distortion.

Thus, it appears, that whenever the action of any of the muscles of the face is associated with the act of breathing, it is performed through the operation of this nerve. I cut a tumor from before the ear of a coachman: a branch of the nerve which goes to the angle of the mouth was divided. Some time after he returned to thank me for ridding him of a

formidable disease, but complained that he could not whistle to his horses.

Of the function of the trigeminus, or fifth nerve, as illustrated by these experiments.

We have seen that when the fifth nerve, the nerve of mastication and sensation, was cut in an ass, the animal could no longer gather his food. In the individual whose face was paralyzed on one side during the excited state of the respiratory organs, there could be observed no debility or paralysis in the same muscles when he took a morsel into his mouth, and began to chew.

By an experiment made on the 16th of March, it was found, that on cutting the infra-orbitary branch of the fifth nerve on the left side, and the *portio dura*, or respiratory, on the right side of an ass, the sensibility to pain on the right side, where the *portio dura* of the seventh nerve was cut, remained entire, while that of the left side was completely destroyed by the division of the fifth. It was also apparent in this experiment, as in the others, that there was the most marked difference in the sufferings of the animal, when these nerves were cut across. The cutting of the fifth nerve gave pain in a degree corresponding with our notions of the sensibility of nerves; but in cutting the *portio dura*, it was not evident that the animal suffered pain at all.

Independently of the difference of sensibility in these nerves, there was exhibited in all these experiments a wide distinction in their powers of exciting the muscles. The slightest touch on the portio dura, or respiratory nerve, convulsed the muscles of the face, whilst the animal gave no sign of pain.

the inference, that the two sets of nerves distributed to the face have distinct functions; even this must prove useful both to the surgeon and physician. To the surgeon it must be useful, in performing operations on the face, as well as in observing the symptoms of disease; but especially to the physician must these facts be important; he will be better able to distinguish between that paralysis which proceeds from the brain, and that partial affection of the muscles of the face, when, from a less alarming cause, they have lost the controuling influence of the respiratory nerve.

Cases of this partial paralysis must be familiar to every medical observer. It is very frequent for young people to have what is vulgarly called a blight; by which is meant, a slight palsy of the muscles on one side of the face, and which the physician knows is not formidable. Inflammations of glands seated behind the angle of the jaw will sometimes produce this. All such affections of the respiratory nerve will now be more easily detected; the patient has a command over the muscles of the face, he can close the lips, and the features are duly balanced; but the slightest smile is immediately attended with distortion, and in laughing and crying the paralysis becomes quite distinct.

The knowledge of the sources of expression teaches us to be more minute observers. The author had lately to watch the breathing of an infant which had been several times restored from a state of insensibility. At length the general powers fell low without any returning fit; insensibility and loss of motion stole over the frame; all but the actions excited by the respiratory nerves ceased; then each act of respiration was attended with a twitching of the muscles of the *ala nasi*,

and of that muscle of the cheek which makes the dimple in smiling. It was then evident that the child could not recover; that all but the system of respiratory nerves had lost their powers; that the features, as far as they were subject to the influence of the other nerves, had fallen.

There are conditions of the lungs, when the patient is in great danger, and yet the inflammation is not marked by the usual signs of pain and difficult motion of the chest. We shall see nothing but the twitching of those muscles of the face, which are animated by the respiratory nerve. We see a certain unusual dilatation of the nostrils, and a constrained motion of the lips, which with the change of voice is just sufficient to give alarm, and indicate the patient's condition. This is a state of the lungs very often produced after severe accidents, as gun-shot wounds, and after great surgical operations.

These circumstances are stated to prove, that the subject of expression is not foreign to medical studies; and certainly, by attention to the action of the muscles of the face, we shall find the views drawn here from the anatomy, farther countenanced. We learn that smiling is an affection of the nerve of respiration on the muscles of the face, and that when laughter shakes the sides, it is only an extended and more convulsive action of the muscles produced by the same class of nerves. When to the paleness and coldness and inanimation of grief, there is added the convulsive sob and the catching of the throat, and the twitching of the lips and nostrils, we discover the same class of nerves to be affected, which, in crying, are only more obviously in operation, producing more violent contractions.

In all the intermediate emotions between these extremes, the varieties of expression in the face are produced by the opposition of the two powers affecting the same muscles ; the one is a voluntary power, by which we restrain the features and conceal emotion; the other is an involuntary power, which cannot be always controuled, but which will sometimes have sway and mingle its influence.

Conclusion.

When the account of the nerves of the throat, neck, and chest, shall be laid before the Royal Society, as those of the face have now been, and when a comparison shall be made of the varieties in nerves corresponding with the changes in the mechanism of respiration in different animals, a juster estimate may be formed of the importance of these observations. Then the same distinctions of structure and function, which are made manifest in the nerves of the face, will be observed in nerves which take an extensive course through the body. We shall be able to distinguish and separate the nerves of respiration, amidst the apparent intricacy of the general system. By cutting across these nerves of respiration, we shall find it possible successively to stop the motions of the several parts, which unite in the act of respiration; not only to stop the motion of the diaphragm, but the motions of the side, of the shoulder, of the larynx or the pharynx, by cutting their respective respiratory nerves. When this is done, they will be left in the exercise of their other functions through their other nerves, and still alive to other excitements, and capable of performing the voluntary motions, though dead to the influence of the heart and lungs.

By thus distinguishing the nerves of respiration, and as it were separating them from the others, we reduce the remaining part of the nervous system to comparative simplicity. The seeming intricacy in the branching of the nerves, their convergence to certain organs from different origins, their re-union and divergence, instead of being a source of confusion, becomes a subject of the highest interest. The re-union and crossing of nerves we now ascertain to be for the purpose of associating the muscles into different classes, for combining them in subserviency to different organs, and placing them under the guidance of a sensibility more certain in its operation than the will.

By these observations, simplicity and arrangement are now the characters of our anatomical demonstrations, and a better foundation is afforded for discovering and comprehending the symptoms of disease.

EXPLANATION OF PLATE XXX.

This engraving represents the nerves of the face, in illustration of the paper on that subject.

A is the *respiratory nerve* of the face, or the portio dura of the seventh, according to the system of Willis.

a. Are branches of this nerve ascending to the temple, where also branches of the fifth nerve may be seen coming out above the jugum.

b. Branches of this nerve ascending to the muscles of the forehead and eyebrow.

c. A large division of the respiratory nerve which goes to the muscles of the mouth and to the integuments of the cheek, where they accompany the blood-vessels that suffuse the cheek.

d. A union of the anterior branches of this nerve, from which pass off several nerves to the muscles of the mouth; and a branch somewhat more remarkable which advances to the muscles of the *ala nasi.*

e. f. g. A superficial plexus of nerves formed on the side of the neck by this respiratory nerve, or *portio dura ;* and the branches of the cervical nerves ; and the phrenic nerve.

h. Other remarkable connections formed between the phrenic nerve, the *descendens noni,* and the respiratory of the face.

B. The frontal branch of the *trigeminus,* or fifth nerve.

C. The infra-orbital division of the same fifth nerve. This branch is large and its subdivisions form a plexus before finally dividing to supply the muscles of the nostril and lip.

D. The third grand division of the fifth nerve, or *mandibulo-labralis,* to the muscles and integuments of the chin and lower lip.

E. The ninth nerve or *lingualis.* Its descending branch will be distinguished, connected with the respiratory of the face, the spinal nerves, and the phrenic. It is also connected with the superior respiratory nerve ; but that nerve is not represented here.

F. The phrenic nerve, or internal respiratory nerve.

G. G. G. Cervical nerves.

XXIX. *Farther researches on the magnetic phænomena produced by electricity; with some new experiments on the properties of electrified bodies in their relations to conducting powers and temperature.* *By Sir* Humphry Davy, *Bart. P. R. S.*

Read July 5, 1821.

I. In my letter to Dr. Wollaston on the new facts discovered by M. Oersted, which the Society has done me the honour to publish, I mentioned, that I was not able to render a bar of steel magnetic by transmitting the electrical discharge across it through a tube filled with sulphuric acid; and I have likewise mentioned, that the electrical discharge passed across a piece of steel through air, rendered it less magnetic than when passed through a metallic wire; and I attributed the first circumstance to the sulphuric acid being too bad a conductor to transmit a sufficient quantity of electricity for the effect; and the second, to the electricity passing through air in a more diffused state than through metals.

To gain some distinct knowledge on the relations of the different conductors to the magnetism produced by electricity, I instituted a series of experiments, which led to very decisive results, and confirmed my first views.

II. I found that the magnetic phænomena were precisely the same, whether the electricity was small in quantity, and passing through good conductors of considerable magnitude; or, whether the conductors were so imperfect as to convey only

a small quantity of electricity; and in both cases they were neither attractive of each other, nor of iron filings, and not affected by the magnet; and the only proof of their being magnetic, was their occasioning a certain small deviation of the magnetized needle.

Thus, a large piece of charcoal placed in the circuit of a very powerful battery, being a very bad conductor compared with the metals, would not affect the compass needle at all, unless it had a very large contact with the metallic part of the circuit; and if a small wire was made to touch it in the circuit only in a few points, that wire did not gain the power of attracting iron filings; though, when it was made to touch a surface of platinum foil coiled round the end of the charcoal, a slight effect of this kind was produced. And in a similar manner fused hydrate of potassa, one of the best of the imperfect conductors, could never be made to exert any attractive force on iron filings, nor could the smallest filaments of cotton moistened by solution of hydrate of potassa, placed in the circuit, be made to move by the magnet; nor did steel needles floating on cork on an electrized solution of this kind, placed in the voltaic circuit, gain any polarity; and the only proof of the magnetic powers of electricity passing through such a fluid, was afforded by its effect upon the magnetized needle, when the metallic surfaces, plunged in the fluid, were of considerable extent. That the mobility of the parts of fluids did not interfere with their magnetic powers as developed by electricity, I proved, by electrifying mercury, and NEWTON's metal fused, in small tubes. These tubes, placed in a proper voltaic circuit, attracted iron filings, and gave magnetic powers to needles; nor did any agitation of the

mercury or metal within, either in consequence of mechanical motion or heat, alter or suspend their polarity.

III. Imperfect conducting fluids do not give polarity to steel when electricity is passed through them; but electricity passed through air produces this effect. Reasoning on this phænomenon, and on the extreme mobility of the particles of air, I concluded, as M. Arago had likewise done from other considerations, that the voltaic current in air would be affected by the magnet. I failed in my first trial, which I have referred to in a note to my former paper, and in other trials made since by using too weak a magnet; but I have lately had complete success; and the experiment exhibits a very striking phænomenon.

Mr. Pepys having had the goodness to charge the great battery of the London Institution, consisting of two thousand double plates of zinc and copper, with a mixture of 1168 parts of water, 108 parts of nitrous acid, and 25 parts of sulphuric acid, the poles were connected by charcoal, so as to make an arc, or column of electrical light, varying in length from one to four inches, according to the state of rarefaction of the atmosphere in which it was produced; and a powerful magnet being presented to this arc or column, having its pole at a very acute angle to it, the arc, or column, was attracted or repelled with a rotatory motion, or made to revolve, by placing the poles in different positions, according to the same law as the electrified cylinders of platinum described in my last paper, being repelled when the negative pole was on the right hand by the north pole of the magnet, and attracted by the south pole, and *vice versâ*.

It was proved by several experiments that the motion de-

pended entirely upon the magnetism, and not upon the electrical inductive power of the magnet, for masses of soft iron, or of other metals, produced no effect.

The electrical arc or column of flame was more easily affected by the magnet, and its motion was more rapid when it passed through dense than through rarified air; and in this case, the conducting medium or chain of æriform particles was much shorter.

I tried to gain similar results with currents of common electricity sent through flame, and in vacuo. They were always affected by the magnet; but it was not possible to obtain so decided a result as with voltaic electricity, because the magnet itself became electrical by induction, and that whether it was insulated, or connected with the ground.*

IV. Metals, it is well known, readily transmit large quantities of electricity; and the obvious limit to the quantity which they are capable of transmitting seems to be their fusibility, or volatilization by the heat which electricity produces in its passage through bodies.

Now I had found in several experiments, that the intensity of this heat was connected with the nature of the medium by which the body was surrounded; thus a wire of platinum which was readily fused by transmitting the charge from a voltaic battery in the exhausted receiver of an air pump, acquired in air a much lower degree of temperature. Reasoning on

* I made several experiments on the effects of currents of electricity simultaneously passing through air in different states of rarefaction in the same and different directions, both from the voltaic and common electrical batteries; but I could not establish the fact of their magnetic attractions or repulsions with regard to each other, which probably was owing to the impossibility of bringing them sufficiently near.

this circumstance, it occurred to me, that by placing wires in a medium much denser than air, such as ether, alcohol, oils, or water, I might enable them to transmit a much higher charge of electricity than they could convey without being destroyed in air; and thus not only gain some new results as to the magnetic states of such wires, but likewise, perhaps, determine the actual limits to the powers of different bodies to conduct electricity, and the relations of these powers.

A wire of platinum of $\frac{1}{220}$, of three inches in length, was fused in air, by being made to transmit the electricity of two batteries of ten zinc plates of four inches with double copper, strongly charged: a similar wire was placed in sulphuric ether, and the charge transmitted through it. It became surrounded by globules of gas; but no other change took place; and in this situation it bore the discharge from twelve batteries of the same kind, exhibiting the same phænomena. When only about an inch of it was heated by this high power in ether, it made the ether boil, and became white hot under the globules of vapour, and then rapidly decomposed the ether, but it did not fuse. When oil or water was substituted for the ether, the length of the wire remaining the same, it was partially covered with small globules of gas, but did not become red hot.

On trying the magnetic powers of this wire in water, they were found to be very great, and the quantity of iron filings that it attracted, was such as to form a cylinder round it of nearly the tenth of an inch in diameter.

To ascertain whether short lengths of fine wire, prevented from fusing by being kept cool, transmitted the whole electricity of powerful voltaic batteries, I made a second indepen-

dent circuit from the ends of the battery with silver wires in water, so that the chemical decomposition of the water indicated a residuum of electricity in the battery. Operating in this way, I found that an inch of wire of platinum of $\frac{1}{220}$, kept cool by water, left a great residual charge of electricity in a combination of twelve batteries of the same kind as those above mentioned; and after making several trials, I found that it was barely adequate to discharge six batteries.

V. Having determined that there was a *limit* to the quantity of electricity which wires were capable of transmitting, it became easy to institute experiments on the different conducting powers of different metallic substances, and on the relation of this power to the temperature, mass, surface, or length of the conducting body, and to the conditions of electro-magnetic action.

These experiments were made as nearly as possible under the same circumstances, the same connecting copper wires being used in all cases, their diameter being more than one-tenth of an inch, and the contact being always preserved perfect; and parts of the same solutions of acid and water were employed in the different batteries, and the same silver wires and broken circuit with water were employed in the different trials; and when no globules of gas were observed upon the negative silver wire of the second circuit, it was concluded that the metallic conducting chain, or the primary circuit, was adequate to the discharge of the combination. To describe more minutely all the precautions observed, would be tedious to those persons who are accustomed to experiments with the voltaic apparatus, and unintelligible to others; and after all,

in researches of this nature, it is impossible to gain more than approximations to true results ; for the gas disengaged upon the plates, the different distances of the connecting plates, and the slight difference of time in making the connections, all interfere with their perfect accuracy.

The most remarkable general result that I obtained by these researches, and which I shall mention first, as it influences all the others, was, that *the conducting power of metallic bodies varied with the temperature, and was lower in some inverse ratio as the temperature was higher.*

Thus a wire of platinum of $\frac{1}{220}$, and three inches in length, when kept cool by oil, discharged the electricity of two batteries, or of twenty double plates ; but when suffered to be heated by exposure in the air, it barely discharged one battery.

Whether the heat was occasioned by the electricity, or applied to it from some other source, the effect was the same. Thus a wire of platinum, of such length and diameter as to discharge a combination without being considerably heated ; when the flame of a spirit lamp was applied to it so as to make a part of it red hot, lost its power of discharging the whole electricity of the battery, as was shown by the disengagement of abundance of gas in the secondary circuit ; which disengagement ceased as soon as the source of heat was withdrawn.

There are several modes of exhibiting this fact, so as to produce effects which, till they are witnessed, must almost appear impossible. Thus, let a fine wire of platinum of four or five inches in length be placed in a voltaic circuit, so that the electricity passing through it may heat the whole of it to

redness, and let the flame of a spirit lamp be applied to any part of it, so as to heat that part to whiteness, the rest of the wire will instantly become cooled below the point of visible ignition. For the converse of the experiment, let a piece of ice or a stream of cold air be applied to a part of the wire; the other parts will immediately become much hotter; and from a red, will rise to a white heat. The quantity of electricity that can pass through that part of the wire submitted to the changes of temperature, is so much smaller when it is hot than when it is cold, that the absolute temperature of the whole wire is diminished by heating a part of it, and, *vice versa*, increased by cooling a part of it.

In comparing the conducting powers of different metals, I found much greater differences than I had expected. Thus six inches of silver wire of $\frac{1}{220}$ discharged the whole of the electricity of sixty-five pair of plates of zinc and double copper made active by a mixture of about one part of nitric acid of commerce, and fifteen parts of water. Six inches of copper wire of the same diameter discharged the electricity of fifty-six pairs of the same combination, six inches of tin of the same diameter carried off that of twelve only, the same quantity of wire of platinum that of eleven, and of iron that of nine. Six inches of wire of lead of $\frac{1}{200}$ seemed equal in their conducting powers to the same length of copper wire of $\frac{1}{220}$. All the wires were kept as cool as possible by immersion in a basin of water.*

I made a number of experiments of the same kind, but the results were never precisely alike, though they sometimes

* Water is so bad a conductor, that in experiments of this kind its effects may be neglected altogether; and these effects were equal in all the experiments.

approached very near each other. When the batteries were highly charged, so that the intensity of the electricity was higher, the differences were less between the best and worst conductors, and they were greater when the charge was extremely feeble. Thus, with a fresh charge of about 1 part of nitric acid, and 9 parts of water, wires of $\frac{1}{220}$ of silver and platinum 5 inches long, discharged respectively the electricity of 30, and 7 double plates.

Finding that when different portions of the same wire plunged in a non-conducting fluid were connected with different parts of the same battery equally charged, their conducting powers appeared in the inverse ratio of their lengths; so, when 6 inches of wire of platinum of $\frac{1}{220}$ discharged the electricity of 10 double plates, 3 inches discharged that of 20, $1\frac{1}{2}$ inch that of 40, and 1 inch that of 60; it occurred to me that the conducting powers of the different metals might be more easily compared in this way, as it would be possible to make the contacts in less time than when the batteries were changed, and consequently with less variation in the charge.

Operating in this way, I ascertained that in discharging the electricity of 60 pairs of plates, 1 inch of platinum was equal to about 6 inches of silver, to $5\frac{1}{2}$ inches of copper, to 4 of gold, to 3.8 of lead, to about $\frac{9}{10}$ of palladium, and $\frac{8}{10}$ of iron, all the metals being in a cooling fluid medium.

I found, as might have been expected, that the conducting power of a wire for electricity, in batteries of the size and number of plates just described, was nearly directly as the mass; thus, when a certain length of wire of platinum discharged 1 battery,* the same length of wire of six times the weight discharged 6 batteries; and the effect was exactly the same, pro-

* A foot of this wire weighed 1.13 grains, a foot of the other 6.7 grains.

vided the wires were kept cool, whether the mass was a single wire, or composed of 6 of the smaller wires in contact with each other. This result alone showed, that surface had no relation to conducting power, at least for electricity of this kind, and it was more distinctly proved by a direct experiment; equal lengths and equal weights of wire of platinum, one round, and one flattened by being passed transversely through rollers so as to have six or seven times the surface, were compared as to conducting powers: the flattened wire was the best conductor in air from its greater cooling powers, but in water no difference could be perceived between them.

VI. I tried to make a comparison between the conducting powers of fluid menstrua and charcoal and those of metals. Six inches of platinum foil, an inch and $\frac{1}{5}$ broad, were placed in a vessel which could be filled with any saline solution; and a similar piece of platinum placed opposite at an inch distance; the whole was then made part of a voltaic circuit, which had likewise another termination by silver wires in water; and solution of salts added, till gas ceased to be liberated from the negative silver wire. In several trials of this kind it was found that the whole of the surface of six inches, even with the strongest solutions of common salt, was insufficient to carry off the electricity even of two pair of plates; and a strong solution of potassa carried off the electricity of three pair of plates only; whereas an inch of wire of platinum of $\frac{1}{220}$ (as has been stated) carried off all the electricity of 60 pair of plates. The gas liberated upon the surface of the metals when they are placed in fluids, renders it impossible to gain accurate results; but the conducting power of the best fluid conductors, it seems probable

from these experiments, must be some hundreds of thousand times less than those of the worst metallic conductors.

A piece of well-burnt compact box-wood charcoal was placed in the circuit, being $\frac{3}{10}$ of an inch wide by $\frac{1}{10}$ thick, and connected with large surfaces of platinum. It was found that 1 inch and $\frac{2}{10}$ carried off the same quantity of electricity as 6 inches of wire of platinum of $\frac{1}{220}$.

VII. I made some experiments with the hope of ascertaining the exact change of ratio of the conducting powers dependent upon the change of the intensity and quantity of electricity; but I did not succeed in gaining any other than the general result, that the higher the intensity of the electricity, the less difficulty it had in passing through bad conductors; and several remarkable phænomena depend upon this circumstance.

Thus, in a battery where the quantity of the electricity is very great and the intensity very low, such as one composed of plates of zinc and copper, so arranged as to act only as single plates of from 20 to 30 feet of surface each, and charged by a weak mixture of acid and water. Charcoal made to touch only in a few points, is almost as much an insulating body as water, and cannot be ignited, nor can wires of platinum be heated when their diameter is less than $\frac{1}{80}$ of an inch, and their length three or four feet; and a foot of platinum wire of $\frac{1}{30}$ is scarcely heated by such a battery, whilst the same length of silver wire of the same diameter is made red hot; and the same lengths of thicker wires of platinum or iron are intensely heated.

The heat produced where electricity of considerable inten-

sity is passed through conductors, must always interfere with the exact knowledge of the changes of their conducting powers, as is proved by the following experiment. A battery of 20 pair of plates of zinc, and copper plates 10 inches by 6, was very highly charged with a mixture of nitric acid and water, so as to exhibit a considerable intensity of electrical action, and the relative conducting powers of silver and platinum in air and water ascertained by means of it. In air, 6 inches of wire of platinum of $\frac{1}{80}$, discharged only 4 double plates, whilst 6 inches of silver wire of the same diameter, discharged the whole combination: the platinum was strongly ignited in this experiment, whilst the silver was scarcely warm to the touch. On cooling the platinum wire by placing it in water, it was found to discharge 10 double plates. When the intensity of the electricity is very high, however, even the cooling powers of fluid media are of little avail: thus I found that fine wire of platinum was fused by the discharge of a common electrical battery under water; so that the conducting power must always be diminished by the heat generated, in a greater proportion as the intensity of the electricity is higher.

It might at first view be supposed, that when a conductor placed in the circuit left a residuum of electricity in any battery, increase of the power of the battery, or of its surface, would not enable it to carry through any additional quantity. This, however, is far from being the case.

When saline solutions were placed in the circuit of a battery of 20 plates, though they discharged a very small quantity only of the electricity, when the troughs were only $\frac{1}{4}$ full, yet their chemical decomposition exhibited the fact of a much

larger quantity passing through them, when the cells were filled with fluid.

And a similar circumstance occurred with respect to a wire of platinum, of such a length as to leave a considerable residuum in a battery when only half its surface was used; yet when the whole surface was employed, it became much hotter, and nevertheless left a still more considerable residuum.

VIII. I found long ago, that in increasing the number of alternations of similar plates, the quantity of electricity seemed to increase as the number, at least as far as it could be judged of by the effects of heat upon wires; but only within certain limits, beyond which the number appeared to diminish, rather than increase the quantity. Thus the two thousand double plates of the London Institution, when arranged as one battery, would not ignite so much wire as a single battery of ten plates with double copper.

It is not easy to explain this result. Does the intensity mark the rapidity of the motion of the electricity? or, merely its diminished attraction for the matter on which it acts? and does this attraction become less in proportion as the circuit, through which it passes, or in which it is generated, contains a greater number of alternations of bad conductors?

Mr. Children, in his account of the experiments made with his battery of large plates, has ingeniously referred the heat produced by the passage of electricity through conductors, to the resistance it meets with, and has supposed, what proves to be the fact, that the heat is in some inverse ratio to the conducting power. The greatest heat however is pro-

duced in air, where there is reason to suppose the least resistance; and as the presence of heat renders bodies worse conductors, another view may be taken, namely, that the excitation of heat occasions the imperfection of the conducting power. But till the causes of heat and of electricity are known, and of that peculiar constitution of matter which excites the one, and transmits or propagates the other, our reasoning on this subject must be inconclusive.

I found that when equal portions of wires of the same diameter, but of different metals, were connected together in the circuit of a powerful voltaic battery, acting as two surfaces, the metals were heated in the following order: iron most, then palladium, then platinum, then tin, then zinc, then gold, then lead, then copper, and silver least of all. And from one experiment, in which similar wires of platinum and silver joined in the same circuit were placed in equal portions of oil, it appeared that the generation of heat was nearly inversely as their conducting power. Thus the silver raised the temperature of the oil only four degrees, whilst the platinum raised it twenty-two. The same relations to heat seem to exist, whatever is the intensity of the electricity; thus circuits of wires placed under water, and acted on by the common electrical discharge, were heated in the same order as by the voltaic battery, as was shown by their relative fusion; thus, iron fusing before platinum, platinum before gold, and so on.

If a chain be made of wire of platinum and silver, in alternate links soldered together, the silver wire being four or five times the diameter of the platinum, and placed in a powerful voltaic circuit, the silver links are not sensibly heated,

whilst all those of the platinum become intensely and equally ignited. This is an important experiment for investigating the nature of *heat*. If heat be supposed a substance, it cannot be imagined to be expelled from the platinum; because an unlimited quantity may be generated from the same platinum, *i. e.* as long as the electricity is excited, or as often as it is renewed. Or if it be supposed to be identical with, or an element of, electricity, it ought to bear some relation to its quantity, and might be expected to be the same in *every* part of the chain, or greatest in those parts nearest the battery.

IX. The magnetism produced by electricity, though with the same conductors it increases with the heat, as I mentioned in my last paper; yet with different conductors I find it follows a very different law. Thus, when a chain is made of different conducting wires, and they are placed in the same circuit, they all exhibit equal magnetic powers, and take up equal quantities of iron filings. So that the magnetism seems directly as the quantity of electricity which they transmit. And when in a highly powerful voltaic battery, wires of the same diameters and lengths, but of which the best conducting is incapable of wholly discharging the battery, are made, separately and successively, to form the circuit, they take up different quantities of iron filings, in some direct proportion to their conducting powers.

Thus in one experiment, two inches of wire of $\frac{1}{30}$ of an inch being used, silver took up 32 grains, copper 24, platinum 11, and iron $8\frac{2}{10}$.

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1820.

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1821.		
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	The European Magazine from July 1820, to July 1821.	The Proprietors.
	The London Magazine, No. 8 to 18.	The Editors.

INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1821.

INDEX.

E

F

G

H

INDEX.

5

INDEX.

INDEX.

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